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ADAPTATION, AFTER-EFFECT AND CONTRAST IN THE PERCEPTION OF CURVED LINES

BY JAMES J. GIBSON

Smith College

INTRODUCTION

The phenomenon with which this paper is chiefly concerned was first observed, quite incidentally, in March, 1929 at the end of an experiment which, in the tradition of Stratton's famous research,[1] was concerned with the formation of new space habits during a period of wearing glasses which distorted the visual field. Instead of a lens with curved surfaces, however, simple prisms were used, one in front of each eye, so that the visual field was shifted bodily to the right (approximately 15°) instead of being inverted. In addition to this shift of the field another, and for the purposes of this discussion more important, distortion was produced by the prisms—the bending of vertical straight lines into curves convex to the left. Horizontal lines were not affected. The vertical edges of doors and windows, for example, all appeared definitely curved in the same direction.

This curvature optically imposed by the prisms is constant in amount in the sense that the curvature of one line is visually congruent with the curvature of any other line in the field. Accordingly, the radius of the curvature varies with the distance of the line from the eye; it is about 9 meters at 3 meters distant, and 3 meters at one meter distant. A

[1] G. M. Stratton, Vision without inversion of the retinal image. *Psych. Rev.*, 1897, 4, 341–360 and 463–481.

somewhat simplified optical explanation of the bending effect is as follows. In the accompanying schematic diagram the ray of light entering the eye from point B the center of a vertical straight line, passes through the prism perpendicularly to its axis and is bent as shown. But the rays of light from *A* and *C*, the ends of the line, pass through the prism diagonally to its axis so that, in effect, they are transmitted through a prism of wider angle and are therefore bent more. Points *A* and *C* will therefore be displaced farther to the right than *B*. In other words, those rays of light from *A*, *B*, and *C*

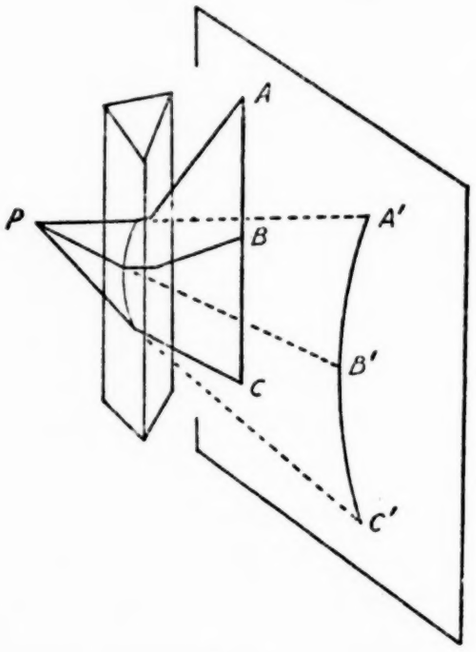

FIG. 1

which are destined to enter the eye at *P*, enter from directions such that when projected on the plane of fixation the points take the position *A'*, *B'*, *C'*. It should be noted that the sheaf of rays which enters the eye takes the form of a curved plane. The rule for the bending effect is therefore that any vertical line will appear convex on the side towards the base, or opposite the apex, of the prism. In the experiment now being described, lines were convex on the left.

The subject of the experiment, *G*,[2] wore the prism glasses continually, except when asleep, for a waking period running

[2] The writer wishes to express his admiration to his former student, Miss Janet Goldschmidt, for her fortitude and careful observation in this original experiment.

through four days and totalling 45 hours. The curvature of the visual world, which at first had been very striking and somewhat disconcerting, was reported on the third and fourth days to be less marked than at the beginning. When the glasses were taken off, and after the series of tests for various spatial reactions had been completed,[3] the subject reported that her visual world was now curved in the opposite direction. All vertical lines and edges now seemed definitely and unmistakeably to be curved with convexity to the *right.* This opposite curvature was more vivid and greater in amount than the prism curvature had been during the last day. The effect persisted strongly the rest of that day and was noticeable during the greater part of the next day.[4]

Here was a phenomenon which clearly demanded further investigation. In the effort to verify the observations of the first subject it was discovered that the curvature after-effect could be obtained with subjects who had worn the glasses for periods as short as an hour. Several experienced observers now served in the experiment, wearing the prisms for periods of an hour and upwards. Without exception all declared that when the glasses were removed vertical lines and edges looked unmistakably curved in a direction opposite to that existing while the prisms were worn. During the training periods these subjects simply walked about or sat, looking at and

[3] The primary purpose of the experiment was to investigate the modifications in auditory and kinæsthetic space habits resulting from a 15° displacement of the visual field. The results, together with those of subsequent experiments on the same problem, are significant but will not be reported in this paper. Prism glasses similar to ours have been used to investigate space perception by Margaret Wooster (Certain factors in the development of a new spatial coördination, *Psychol. Monog.*, 1923, 32, no. 4) and by David Camp Rogers in an unpublished research. In these investigations however the glasses were worn intermittently rather than continuously as in the present experiment.

[4] Another and no less striking after-effect of the prisms was reported but has not yet been confirmed. While wearing the prisms, as might be expected, dark lines on a brighter ground were bounded vertically by narrow bands of color, *i.e.* half-spectra, red on the right and blue on the left. This phenomenon was reported to be less marked at the end of the three-day period. When the glasses were removed, G stated that the colored bands were now seen with renewed vividness but that the effect was reversed, red now being on the left and blue on the right. The phenomenon persisted for several hours. For obvious reasons, explanation cannot be made in terms of ordinary negative after-images.

feeling objects, and in general becoming acquainted with the curved prism world.

The Experiments. Series I

The phenomenon having been verified to this extent, an experiment was planned with two ends in view, first, to measure the after-curvature, and, second, to test for and measure anything in the nature of adaptation during the period of wearing the prisms. It seemed very probable that if a straight line looked slightly curved to the right when the prisms were removed, this was due to the fact that a line curved to the left at the beginning of the period looked slightly less curved to the left at the end of the period. Or, in terms of an analogy with color vision, if there were a "negative after-image" of curvature there must also be a fading or desaturating of curvature during the period.

The Prism Glasses.—The glasses used in these experiments consisted of a pair of ordinary optometrist's trial prisms, mounted in trial frames, with the apex of both prisms towards the right. The angle of the prisms being close to 26°, the amount of deflection of a ray of light in the center of the visual field (angle of minimum deviation) was approximately 14°.[5] The glasses were set in a mask, at first of cardboard and cloth, later of leather, which fitted closely around the forehead and below the eyes, fastening behind the head. This device effectively excluded all light except that passing through the prisms.

Procedure During the Period.—Each of the subjects in Series I wore the prisms for a period of one hour. During the hour each subject underwent a fairly uniform program of training, which consisted chiefly in looking at and running the fingers along vertical lines and edges such as that of a meter stick. It was anticipated at the time of these experiments that a possible explanation for the 'after-curvature' might be found in terms of conflict between vision and kinæsthesis. Crudely, the hypothesis was that if a meter stick for example were seen as curved but felt as straight, adjustment would occur in the direction of less visual curvature. To facilitate such adjustment, should there be any, the training described was made to occupy intervals totalling about half of the hour period.

It was discovered, however, that in actual fact the kinæsthetic perception, in so

[5] The formula used for the calculation is $\delta = (\mu - 1)\alpha$, δ being the angle of deviation, μ being 1.53, and α being the angle of the prism.

far as it was consciously represented, did *not* conflict with the visual perception. When a visually curved edge such as a meter stick was felt, it was felt as *curved*. This was true as long as the hand was watched while running up and down the edge. If the eyes were closed or turned away, the edge of course felt straight, as it in reality was. This dominance of the visual over the kinæsthetic perception was so complete that when subjects were instructed to make a strong effort to dissociate the two, *i.e.* to 'feel it straight and see it curved,' it was reported either difficult or impossible to do so.[6]

The intervals of the test period not taken up by the activities just described were occupied with simple exercises designed to give practice in coördinating movement with the new visual space, which it will be remembered was shifted some 15° to the right. Objects, that is to say, were seen 15° to the right or where they really were. At first, therefore, errors to the right were made in reaching for or pointing to objects, the subject walked to the right of where he wished to go, bumping into objects and door frames on the right, etc. By the end of the hour, however, these errors had either decreased greatly in extent or completely disappeared.[7]

In order to measure the anticipated after-effect of curvature and also the possible adaptation to curvature, a 30 cm vertical straight line was drawn on a large sheet of white cardboard, which was then mounted on a screen and set up on a table with a head-rest clamped to its edge. The distance from the cardboard screen to the observer's eyes was 40 cm, so that the line extended over a visual angle of approximately 42°. To the right and left of the mid-point of this line there was a scale, in 4 mm units. With the aid of this scale, it was anticipated that, after the removal of the glasses, any apparent curvature of the line could be estimated with reasonable accuracy to the quarter of a unit (1 mm). This expectation seemed reasonable, since the subjects had heretofore reported that the 'after-curvature' was sufficiently stable to allow of a fairly careful estimate of its extent in terms of the apparent displacement to right or left of the center of a straight line.

For the measurement of the curvature adaptation during the period a somewhat more precise method proved to be possible. Immediately after putting on the prism glasses the subject sat at the table and, adjusting his head in the head-rest, faced the screen. The line appeared definitely convex to the left,[8] the center of the line being displaced about 10 mm. The amount of this displacement of the center of the line could be estimated on the scale without difficulty, but could be even more precisely determined in the following manner. S was given a slender flexible black fibre rod, 2 feet long. This rod, of course, likewise appeared convex to the left in prism vision, but it could be held against the screen touching the right edge of the line, with one nand at either end, and then could be bent convexly to the right until it looked perfectly straight. Bent in this way, the rod served as a straight-edge in an otherwise curved world and could be used to measure the apparent displacement of the center of the line with some accuracy. The top and bottom of the 'curved' line being in juxtaposition with the left hand edge of the 'straight' rod, the displacement at the center

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[6] The fact of visual dominance under these conditions was noticed and reported by G, the original observer, during the 4 day experiment. She has since carried out a detailed investigation of this phenomenon.

[7] For a quantitative study of the improvement in localizing movements of the hand while wearing prism glasses, see Wooster, *op. cit.*, 13–47 and 63–67.

[8] During all the experiments of Group I, the prisms were worn with apex to right.

could be read off on the scale by the experimenter. The apparent curvature of the line was measured by this method in millimeters at the beginning of the period (Test I) and at the end of the period (Test II). Any adaptation which had occurred would show up as a decrease from Test I to Test II.

Results.—Ten subjects were used in this series of experiments. Only two of these had ever had any previous experience in psychological observation, but nevertheless no one had any difficulty in observing the phenomenon, and all without exception reported a marked negative after-curvature. Five of the subjects had heard of the effect and expected something similar to what they saw; the remaining five were completely in ignorance of the purpose of the experiment and the after-curvature was unexpected and was usually reported as surprising. As regards adaptation, three subjects reported spontaneously at some time during the hour that the curvature seemed somewhat less than at the beginning.

The results are given in Table I. Column 4 shows the estimates on the scale, of the negative after-curvature, and column 3 the decrease in perceived curvature from beginning until end of the hour (Test I minus Test II). Both after-

TABLE I

ADAPTATION AND NEGATIVE AFTER-EFFECT AS MEASURED IN MILLIMETERS OF DISPLACEMENT AT THE CENTER OF A 30 CENTIMETER LINE. CURVATURE PRODUCED BY PRISMS. TRAINING PERIOD OF ONE HOUR

Subject	Test I	Test II	Decrease from I to II (Adapt.)	Test III (Neg. Aft.–Effect)
A	10 left	7 left	3 right	2 right
B	7	6	1	1
C	9	8	1	1.3
D	9	8	1	1
E	10	9	1	0.5
F	9	8	1	1
G	9	7	2	2
H	10	8	2	2
I	9.4	9.2	0.2	2
J	9	7.7	1.3	2
Avg's.	9.14 left	7.79 left	1.35 right	1.48 right

Entries in fractional parts of a millimeter are averages of two or three measurements in cases where the subject was not satisfied with the first measurement and repeated it. The individual differences in perceived curvature shown under 'Test I' are due to slight variations in the fixation distance from subject to subject.

effect and adaptation occurred consistently and in the expected direction for all subjects. The averages of the two are respectively 1.48 mm and 1.35 mm. Allowing for the rather crude technique employed in this first series of experiments, the agreement is striking. It seems to indicate, as was expected, that the after-effect is of the same magnitude as the adaptation, and that therefore the two phenomena are essentially the same. In other words the curved line of Test I was decreased in curvature to the left at the end of the hour by as much as the straight line was then curved to the right.

A diagram may be of use here in visualizing the situation. Figure 2 shows schematically the relations between the

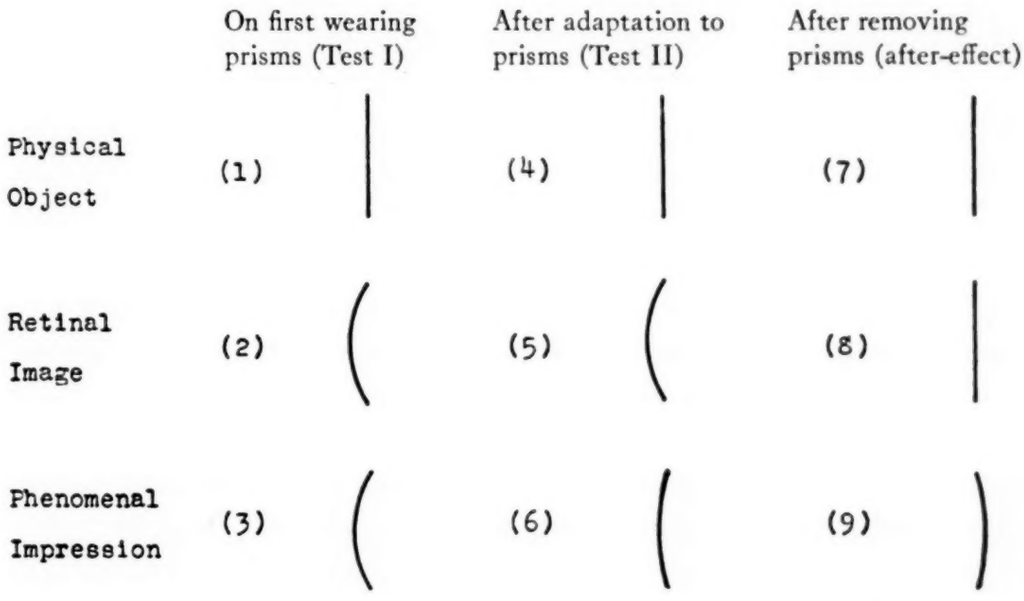

FIG. 2

physical object, the retinal image, and the phenomenal impression. The inversion of the retinal image is left out of account but its inclusion would not essentially alter matters. It is clear that if the phenomenal impression (6) is the correllate of the retinal image (5), then (9) should be the correllate of (8). In a sense, decrease in curvature to the left is the same thing as increase in curvature to the right. In this sense, the difference between (5) and (6) is the same as that between (8) and (9). Everything is clear in the diagram,

then, except the question how (6) comes to be the correllate of (5). In other words, whatever it is in the organism which underlies what we have called the 'phenomenal impression,' be it cortical excitations or implicit responses, it changes in the course of an hour in such a way as to make (3) change to (6). Our next problem was to explain how this change came about.

Theoretical.—At this stage of the investigation, two very different types of explanation for the decrease in perceived curvature could be formulated.

1. It was possible that the explanation lay mainly in conflict between one type of experience and another. The subject could run his fingers along the edge of a meter stick and thus bring kinæsthetic cues into conflict with visual. Presumably, the visual impression would be modified as a result. Such a discrepancy between data from different sense departments has been considered the sufficient condition for changes of the type observed by Stratton, Wooster, Young, and Ewert.[9] It could be objected, of course, that these and other researches have never proved any significant tendency for visual perceptions to swing into line with conflicting perceptions from other senses. Indeed it could be shown that the opposite has always been indicated. And likewise it could be pointed out that the kinæsthetic and visual perceptions actually did not conflict in the prism situation, the visual perception dominating unmistakably. Despite these objections, however, it was considered that the explanation was a plausible one under the circumstances.[10]

A less specific hypothesis presented itself as a variant of the above. Since the subject knew that the lines which he

[9] G. M. Stratton, *op. cit.;* M. Wooster, *op. cit.;* P. T. Young, Auditory localization with acoustical transposition of the ears, *J. Exper. Psychol.*, 1928, 11, 399–429; P. H. Ewert, A study of the effect of inverted retinal stimulation upon spatially coördinated behavior, *Gen. Psychol. Monog.*, 1930, 7, 177–363.

[10] On first thought it might also appear that eye movement kinæsthesis would conflict with the visual perception. But it must be remembered that when the eyes behind the prisms follow a line up and down, they travel a curved path, since the curvature is imposed upon the sheaf of light rays before they enter the eye. Hence there is no discrepancy between eye movement and visual impression, and conflict between them is impossible.

saw as curved were 'really' straight, this knowledge itself might in some fashion modify the perception; that is, it might cause the subject to see a curved line as less curved. Precisely how this could occur was not clear.

2. The explanation for the decrease in curvature could be sought within the perceptual process itself, or in other words, the phenomenon might be akin to sensory adaptation. It is known that color perceptions in the course of adaptation approach gray, and it is at least conceivable that there is a general tendency for curved lines to undergo an adaptation in which they approach rectilinearity. The straight line is in a sense the average or mean of all lines, the neutral type from which the positive types deviate, in somewhat the same way as the achromatic series is in an average or neutral position in relation to the other colors. Accordingly the possibility had to be considered that curvature perceptions as well as color perceptions, when long continued, tend to approach their neutral type or norm.

The next step was to seek evidence as to which of the two explanations could best be applied. The first of the two could easily be subjected to an experimental test. If conflict between the visual impression and the kinæsthetic feel were the sole cause of the reduction in apparent visual curvature, then we should need only to eliminate all movements of the hands during the test period, and the after-curvature should fail to appear. This experiment was tried with instructions simply to sit motionless with the head in the head-rest and look at the vertical line on the cardboard screen. The line appeared curved to the left. Although under these conditions, the prisms were worn for only 10 minutes, S reported a definite after-curvature convex to the right, which was comparable in amount to the effects previously obtained. This result was immediately verified with two other experienced subjects.

The conclusion was inescapable therefore that conflict between vision and kinæsthesis could not explain the curvature adaptation. Adaptation occurred in the absence of any possibility of such conflict. Furthermore, as will be seen

from Table II later, it occurred in slightly larger amount when such conflict was ruled out than when conditions favorable to conflict were maintained during the adaptation period. The main body of the first hypothesis had therefore to be rejected. Even the possibility next mentioned could now be tested experimentally. If knowledge that the curved line was 'really' straight had anything to do with the phenomenon, then there should be no after-effect from looking at an actually curved line which not only appeared curved but was thought of as curved. For the only essential difference between the perception of a directly seen curved line, and the perception of a straight line visually curved by prisms in front of the eyes, is that in the later situation, the line is known to be 'really' straight. A 30 cm curved line similar in other respects to the straight line heretofore used was drawn on a sheet of cardboard and this was placed in front of the old screen. The extent of curvature was the same as that induced by the prisms—approximately 10 mm, convex to the left. As before, the subject was instructed simply to look at the line, this time of course with undistorted vision, for 10 minutes. At the end of this time, the curved line was removed and the subject was told to report the appearance of the straight line on the screen underneath. Again the subject reported a definite after-curvature convex to the right, slight but unmistakable. The experiment was repeated with a second and a third subject. The reports were just as definite as before. The straight line looked curved to the right, the edge of the door looked curved, a meter stick held vertically looked curved. Evidently, the first hypothesis had to be rejected in its entirety. Only the second hypothesis remained.

This discovery showed the adaptation phenomenon in an entirely new light. Evidently the fact of optical distortion had essentially nothing to do with it. Subject to verification by future experiments, the principle could be formulated that *a curved line, when perceived for any considerable period of time, becomes phenomenally less curved than it was at the beginning of the period, and at the end of the period an objectively straight*

line will seem curved in the opposite direction. This fact holds whether the curvature is actually in the object, or is induced by the distorting effect of prisms. The immediate inference was that we were dealing with a phenomenon strikingly similar to simple, sensory adaptation of the type exhibited by color and perhaps temperature. We apparently have to look within the perceptual process itself for the explanation of the adaptation.

A number of aspects of this newly discovered adaptation to 'real' curvature demanded immediate investigation. It was first demonstrated that whether the curved line were convex to right or left made no difference; the after-effect was always negative, *i.e.* convex on the opposite side. Next it was proved from the reports of several subjects that the after-effect could be observed just as well if the curved line and the subsequently seen straight line were horizontal or diagonal instead of vertical. Next an informal search was made for the degree of curvature which would give the strongest after-effect. A strongly curved line did not seem at this time to give a significantly better effect than the rather weak curvature induced by the prism glasses. Nevertheless, 25 mm of displacement at the center of a 30 cm line (instead of 10) was most frequently used in subsequent experiments.

It was also important to discover whether movement of the eyes during the adaptation period had anything to do with the occurrence of the after-effect. Heretofore the subjects had been instructed merely to 'look at' the curved line during the 10 minute period. This usually meant what we shall call 'inspection' of the line, namely, that the eyes moved freely up and down it and conditions were thus similar to those existing when the prisms were worn for an hour period. The effect of fixating the center of the line as steadily as possible for 10 minutes was now tested. Perfect fixation could not always be maintained, but was usually approximated. The after-curvature appeared as definitely as it had under past conditions. This after-effect was compared with that resulting from a 10 minute period in which the eyes were moved up and down the line regularly. The latter procedure seemed to give a slightly stronger effect, but the

difference was not marked. Evidently neither the presence nor the absence of eye movements is a necessary condition for the occurrence of after-curvature.

It was also roughly determined that the use of a number of parallel curved lines side by side gave no stronger after-effect than one line alone, and that conditions were essentially the same when one used a line composed of dots, as when one used a continuous line. It was established that the phenomenon was unchanged whether one used a white line on black or a black line on white. If the former were used for fixation the after-effect would show up on the latter and *vice versa*. Likewise a thin line could be substituted for a thick line without destroying the effect.

The optimal length of time for fixating or inspecting the line was not determined, but it was found that any period about one or two minutes gave an unmistakable effect. As for the length of time during which the negative after-effect persisted, it was found to be somewhat difficult to determine and the reports were rather variable. After 10 minutes of fixation or inspection the effect usually persisted for at least a minute, and in some cases it was reported as noticeable after 5 minutes or more.

In the effort to obtain quantitative data, two new series of experiments were next undertaken, Series II and III.

Series II. Adaptation to Prism Curvature with 10 Minute Inspection Period

Method.—The experiments of Series II were performed in order to verify and measure the adaptation and after-effect of prism curvature using the brief inspection period. The apparatus was the same as that used in Series I, except that the 30 cm line had no scale to right and left of its mid-point. S wore the prisms for only 10 minutes and sat motionless with his head in the head-rest. The apparent curvature at the beginning and at the end of the period was measured as before by having S use a flexible rod as a 'straight-edge.' The amount of displacement at the center of the line, *i.e.* the distance between line and rod, was now measured by E with a small millimeter rule, instead of allowing the subject to read off the displacement on the scale. This procedure kept S in ignorance of any measurements and eliminated a great source of distraction. For greater reliability, 5 measurements of the curvature were taken at the beginning, and 5 more at the end of the 10 minute period. On removing the prisms, the after-curvature was measured by having S mark with a pencil the extent of displacement at the mid-point of a 30 cm straight line similar to the inspection line. Ten subjects were used, 8 of them being new to the experiment, and 5 being completely ignorant of the phenomenon in question.

Results.—The data are given in Table II. It may be noted that the curvature induced by the prisms, as measured before adaptation had set in (Test I), varies from 10 to 12.6 mm. These measures are somewhat larger than those recorded in Series I, where less care was taken to keep the 40 cm fixation distance constant. The average decrease in curvature during the period is 1.65 mm, and the average negative after-curvature is 1.67 mm. The agreement, as in Series I, is very close and serves only to strengthen the conclusion that adaptation and negative after-effect are aspects of the same phenomenon. It should also be noted that the amount of effect is, if anything, slightly greater than that occurring in Series I, where visual-kinæsthetic conflict could have entered into the situation. This fact lends weight to the conclusion that such conflict has no essential part to play in explaining the effect.

TABLE II

ADAPTATION AND NEGATIVE AFTER-EFFECT AS MEASURED IN MILLIMETERS OF DISPLACEMENT AT THE CENTER OF A 30 CENTIMETER LINE

Subject	Test I	Test II	Decrease from I to II (Adapt.)	Test III (Neg. Aft.-Effect)
A	10.5 left	9.5 left	1.0 right	1.5 right
B	11.3	10.5	.8	1.0
C	10.7	8.8	1.9	2.0
D	11.7	10.4	1.3	1.0
E	12.6	10.2	2.4	2.0
F	11.9	9.3	2.6	1.7
G	11.9	9.7	2.2	1.5
H	10.0	8.8	1.2	1.5
I	11.4	9.5	1.9	3.0
J	11.6	10.4	1.2	1.5
Avg's	11.36 left	9.71 left	1.65 right	1.67 right

Mean variations for the averages given under Test I and Test II are all below 0.9 mm.

In order to furnish a control for the results of this experiment, two subjects went through the following procedure. Instead of inspecting for 10 minutes a straight line optically curved by the prisms, they looked at a curved line which was optically made to appear straight by the prisms. The result of inspecting this phenomenally straight line was, as might

have been expected, that no after-effect whatever appeared. Evidently, then, it may be concluded that it is the fact of a visual curvature, not the prism situation itself, which is essential to the effects being investigated.

Series III. Adaptation to Real Curvature with 10 Minute Inspection Period

Method.—The next step was to verify and measure the curvature adaptation and after-effect resulting from a 10 minute inspection of a line which was actually curved. No prism glasses were worn. In other respects the procedure was the same as in earlier series. The 30 cm curved line was convex to the right, with 27 mm of displacement at the center.[11] The perceived curvature at the beginning of the period (Test I) was measured by holding the flexible rod already mentioned in what seemed a perfectly rectilinear position, just touching the two ends of the curved line on its concave side. E then measured the displacement at the center with a millimeter rule. This procedure was similar to that used in Series II, except that here of course the rod did not have to be strongly bent in order to appear straight. Measurements were repeated 5 times. The perceived curvature at the end of the period (Test II) was measured in the same way. By this time, however, the subject's perception of 'straightness' had changed, owing to the adaptation, and hence the flexible rod had to be bent slightly in the same direction as the inspection line in order for it to appear straight. The measured displacements of Test II were accordingly less than those of Test I. The after-curvature (Test III) was measured by marking with a pencil the apparent displacement at the center of a 30 cm straight line. During the inspection period, S was instructed simply to look at the curved line, moving the eyes up and down the central portion only. He was not to look away to right or left.

Results.—Note first that in Table III under 'Test I,' the measurements of the displacement of the center of the curved line are all (with one exception) under 27 mm. The fact was that the flexible rod, even before adaptation could occur, had to be very slightly bent in the same direction as the line in order to appear straight. Or in other words, an objectively straight line set alongside of a curved line will appear curved in the opposite direction. The fact is pointed out here as foreshadowing the results of the next series of experiments on simultaneous curvature contrast.

[11] Since the radius of such a curved line is large the most satisfactory method of drawing it on a sheet of cardboard is the following. Three points are marked out, the middle point being displaced to the desired extent. A flexible straightedge such as an architect's adjustable curve rule is bent into line with the three points and the ink line is ruled with a wide pen. The resulting curve is not usually perfectly circular but its departure from circularity is so slight as to be insignificant. In any case, the form of the curved line has no influence on the results.

TABLE III

ADAPTATION AND NEGATIVE AFTER-EFFECT AS MEASURED IN MILLIMETERS OF DISPLACEMENT AT THE CENTER OF A 30 CENTIMETER CURVED LINE WITH AN OBJECTIVE DISPLACEMENT OF 27 MILLIMETERS

Subject	Test I	Test II	Decrease from I to II (Adapt.)	Test III (Neg. Aft.-Effect)
A	26.1	24.6	1.5	1.5
B	26.6	23.7	2.9	2.5
C	26.5	24.7	1.8	1.5
D	27.9	26.7	1.3	1.0
E	26.2	23.9	2.3	2.5
F	26.8	25.4	1.4	2.0
G	26.5	25.1	1.4	1.5
H	25.4	24.8	.6	.7
I	25.4	24.5	.9	1.0
J	26.6	24.7	1.9	2.5
Avg's	26.4	24.8	1.60	1.67

Table III shows the same type of results as did Tables I and II. Adaptation and after-effect are again approximately equal (1.60 and 1.67 mm), as indeed they must be. The only new fact to be noted is that the adaptation and after-effect shown here are almost exactly the same as the adaptation and after-effect obtained in the corresponding prism situation (1.65 and 1.67 mm). The conclusion can no longer be in any doubt that a curved line, when inspected for 10 minutes, becomes apparently less curved by the same amount as a straight line thereafter appears curved in the opposite direction.

SERIES IV. THE CONTRAST PHENOMENON

Since curvature perception showed such a striking similarity to color perception with respect to adaptation and after-effect, it seemed possible that there might be a further similarity with respect to simultaneous contrast.[12] In color perception, a stimulus area of one quality surrounding a small stimulus area of neutral quality induces in the latter the opposite quality. Blue surrounding gray induces yellow. To test for contrast in curvature perception, therefore, we

[12] For this suggestion and for many others of great value during the course of the research, the writer is indebted to Dr. Alexander Mintz, formerly of the Smith College Research Laboratory.

have only to surround a straight line with curved lines. This was tried and as may be seen in Fig. 3, an opposite curvature is induced in the straight line. The effect, as in the case of adaptation, is slight but nevertheless definite. It is

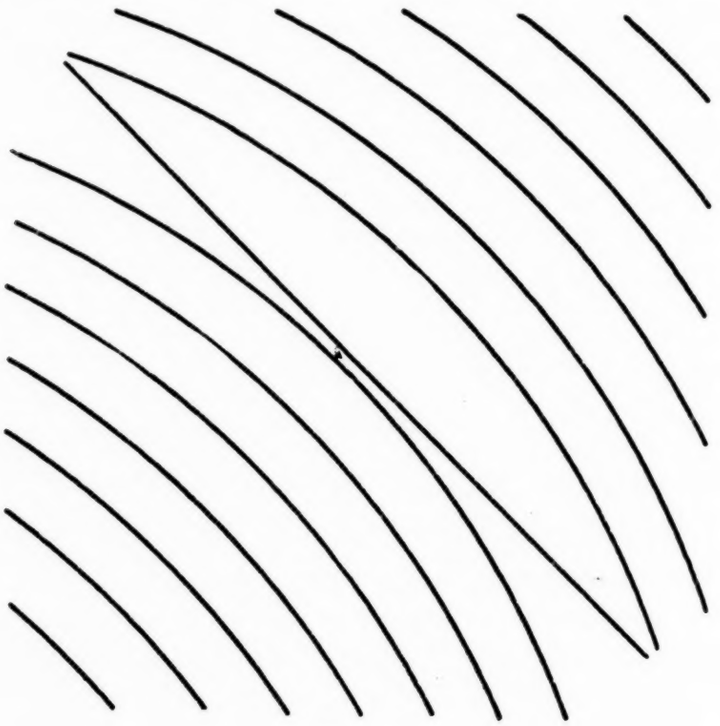

FIG. 3

somewhat greater in extent when the straight line is drawn so as to cut across the curved lines as in Fig. 4.

In an attempt to measure roughly the extent of the induced curvature, the lines shown in Fig. 3 were drawn on a square

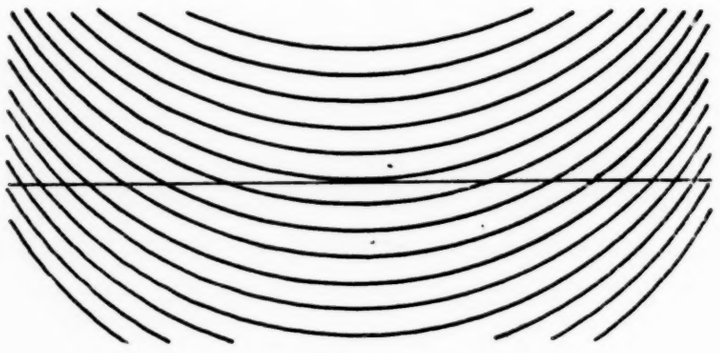

FIG. 4

of cardboard, the length of the lines being 25 cm and the curvature displacement 10 mm. Under these conditions there was an induced curvature on the straight line between 1 and 2

mm in extent, for every one of 20 subjects. This result was arrived at by (1) having S compare the contrast line with lines of zero, 1 mm and 2 mm curvature on a separate piece of cardboard—either the 1 or the 2 mm curve was always picked as the best match; (2) having S compare the contrast chart with a second chart, identical except that the contrast line was curved 2 mm in the same direction as the inducing lines. This curved line for most subjects appeared straight, and for all subjects appeared much nearer to straight than the actually straight line.[13]

Series V. Adaptation to Curvature in Kinæsthetic Perception

During the experiments of Series I it was noted that subjects, while wearing the prisms, reported that a straight vertical edge not only looked but felt curved when the fingers were run along it. If the eyes were closed, however, the edge felt straight. It could be concluded from these facts not only that the visual impression dominated the kinæsthetic, but also that kinæsthetic perceptions of rectilinearity and curvature are clear and discriminable experiences. In view of this apparently high degree of acuity, and to see whether or not an analogy held between visual and kinæsthetic perception, it was desired to find out whether adaptation and after-effect took place in kinæsthetic curvature perception unaccompanied by vision.

A large piece of heavy cardboard was prepared with the right hand edge curved convexly. The edge was 30 cm long and the curvature displacement was 2 cm. The cardboard was presented to a blindfolded subject and he was told to spread slightly the fingers of the right hand and run them from top to bottom of the edge, continuing for a period of 3 minutes. At the end of this time, the subject was given a straight card-

[13] Geometrical illusions closely related to what is here called curvature contrast may be found in the literature. See *e.g.* K. Bühler, *Die Gestaltwahrnemungen*, 1913, 39–40, and H. Werner, Studien über Strukturgesetze, I Über Strukturgesetze und deren Auswirkung in den sogenannten geometrisch-optischen Täuschungen, *Zschr. f. Psychol.*, 1924, **94**, 257–262. Figure 4 of the present study is somewhat similar to the Hering illusion.

board edge to feel in the same way. Of 15 subjects who were given this test all stated voluntarily during the 3 minute period that the convex curvature seemed to decrease, and all reported subsequently that the straight edge felt definitely concave.

As a further check on this new phenomenon, another convexly curved cardboard edge was made with only 6 mm of displacement. This slightly curved edge and the straight edge were presented to subjects as paired comparisons, order being haphazard. Under these conditions all subjects (10) could distinguish with 100 percent accuracy which edge was straight and which was curved. If now the subject were instructed to feel the strongly curved convex edge for 3 minutes and then to feel the two edges described, the straight edge was always reported concave (*i.e.* curved inwards) and the slightly convex edge was usually reported straight or occasionally even slightly concave. Without exception the convex line was declared more nearly straight than the straight line.

Curvature adaptation and curvature after-effect, therefore, may be demonstrated in kinæsthetic [14] as well as in visual perception.

Series VI. Negative After-Effect with a Bent Line Instead of a Curved Line

It is clear that a straight line may become non-rectilinear in one of two regular ways, by being curved, or by being bent into an angle. In the former case the departure from rectilinearity is distributed over the entire length of the line; in the latter it is concentrated in one point. Of the two types of non-rectilinearity, then, does the angle, as well as the curve, manifest the phenomenon of adaptation and negative after-effect? This was the problem set for the present series of experiments.

A vertical line bent at the middle into an obtuse angle was used for fixation. As in previous experiments, it was 30 cm long and was exposed at a distance of 40 cm from the eyes.

[14] It is true that a component of touch experience enters into the feeling of an edge. But it is doubtful if this touch component has anything to do with the curvature perception as such.

The displacement in mm of the center or apex, rather than the number of degrees in the angle, will be stated hereafter in order to maintain the convention adopted for curved lines.

Preliminary experiments with a bent line of 23 mm displacement demonstrated that the negative after-effect showed itself clearly and unmistakably. After 10 minutes of fixation on the apex of the obtuse angle, a straight line seemed definitely bent the opposite way, the bend appearing to move up and down the line as the fixation point shifted.

When, however, the bent line was *inspected* during the ten minute period, *i.e.* if the eyes were moved up and down, the situation was somewhat more complicated. If the eye movements were limited to the middle part of the line close to the bend the after-effect showed itself with fair strength, but if the eyes were allowed to roam over the entire line little or no after-effect could be seen. These facts are intelligible on the supposition that adaptation occurs only in a region of actual non-rectilinearity, *i.e.* only in the neighborhood of the apex and not on the straight sides of the angle.

In an attempt to measure the amount of after-effect more accurately than had hitherto been possible, an apparatus was constructed. It consisted essentially of an adjustable line pivoted in the middle by a pin which was fixed to a sliding strip. (See Fig. 5.) By moving the latter, the line could be bent either to the right or left or made straight. A millimeter scale on the sliding strip made it possible to read the displacement of the center of the line to a fraction of a millimeter. The line itself consisted of two narrow strips of dark gray cardboard, 5 mm wide; the pivot in the middle was an ordinary nickel-plated pin, the head of which served as fixation point. The rest of the apparatus was made of heavy white cardboard.

The procedure adopted for measuring the after-effect was as follows. The center of the line was set at 15 mm from the rectilinear position, either to right or left, and S fixated the pinhead for 5 minutes.[15] At the end of the period S closed his

[15] Adequate fixation for longer than 5 minutes is very difficult and fatiguing. With the repeated trials necessary in this series of experiments, longer fixations than this could not be demanded of subjects.

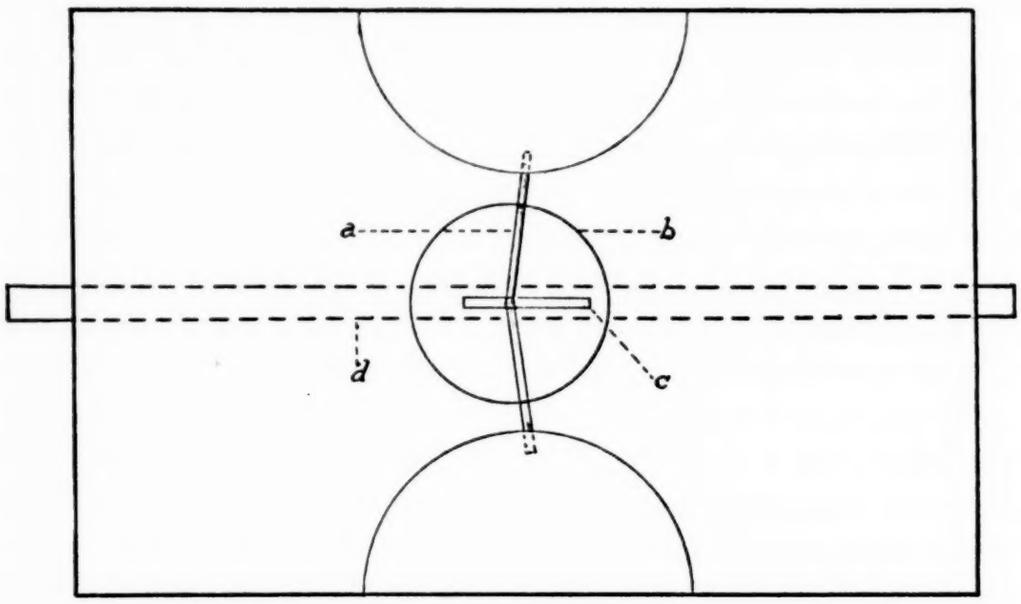

FIG. 5. Apparatus for measuring the after-effect of a bent line.

a—gray cardboard strips constituting the adjustable line; *b*—white circular screen, moving with the line, which covers slot *c*; *c*—slot in cardboard screen, along which pivot can move; *d*—sliding strip of cardboard running in grooves behind the screen.

eyes for 2 seconds while E set the line back to straight. S then opened his eyes and again fixated the pinhead.[16] If the line looked straight S was to so report; if it appeared bent he was to adjust it until it did appear straight. Ten seconds were allowed for this adjustment. No reports of 'straight' occurred; the S always set the line at a slight bend in the same direction as the fixation line. This displacement was of course the amount just necessary to counteract the negative after-effect. It was read from the scale in millimeters, fractions being estimated in tenths.

By this method and under these conditions, the negative after-effect was measured over a series of trials. The averages for 6 subjects are as follows: subj. F, 3.75 mm; subj. G, 2.2 mm; subj. J, 3.0 mm; subj. M, 4.3 mm; subj. S, 2.5 mm; subj. V, 3.2 mm.[17] These figures are averages of from 4 to 8 single

[16] It was discovered that, when S simply adjusted the line to 'straight,' starting from its bent position during fixation, a large constant error confused the results. That is, control experiments showed that when without previous fixation one merely attempts to straighten the bent line, one never moves it far enough.

[17] These averages, that of subj. F excepted, are given again in Table IV a few pages later on.

trials. The mean variation for any subject did not exceed 0.9 mm. The average for all subjects was 3.17 mm.

It is evident that there are individual differences in the amount of after-effect obtained under these conditions, one subject obtaining an effect of 4.3 mm, while another has only 2.2 mm. It is the impression of the experimenter from observations on many subjects that the differences between individuals show up quite consistently. It can also be noted from the above results that the amount of after-effect is greater than that found by using the earlier and cruder methods of measurement employed with the curved lines. This is probably to be ascribed to the fact that the measurement was here taken within 12 seconds after the end of the fixation period. Although the effect does not wholly die away for some time, it seems to be strongest in the first moments after fixation has ceased. It is similar in this respect to after-images of color. It is possible, of course, that this is not the whole explanation. Adaptation and after-effect may be stronger with a bent line than with a curved line. But the question can be decided only when a curved line apparatus, similar in principle to the one described for bent lines, is used. Technical difficulties have delayed the construction of such a device until the present.

Series VII. The Limitation of the After-Effect to the Stimulated Area of the Visual Field

The analogy between color adaptation and curvature [18] adaptation, so far as it has been developed, might lead to the expectation that the curvature after-effect should be limited to the specific area of the visual field which has undergone adaptation. It is well known that negative after-images of color appear with considerable precision, only in the area previously occupied by the stimulus object. The question is then whether or not the negative after-curvature will appear on an objectively straight line only when that line occupies

[18] Hereafter, for lack of a better word, 'curvature' will be used to include both the curve and the angle type of deviation from rectilinearity. 'Non-rectilinearity' is too ponderous a word, and such terms as 'flexion' or 'aberrance' are too vague.

approximately the same visual area as did the curved stimulus line.

Several experiments were performed which indicated that the curvature after-effect is so limited. The first of these was an experiment which demonstrated that two curved lines at right angles to each other induced their own after-effects independently on two straight lines at right angles to each other. The results were the same if the two curved lines, and the straight lines subsequently seen, intersected at their mid-points. If a pattern or grouping of curved lines were fixated, and subsequently a similar pattern of straight lines were substituted for it, each straight line showed the particular after-effect appropriate to its corresponding curved line.[19] In this procedure, of course, the fixation points were so placed as to bring the straight lines into the approximate visual positions previously occupied by the curved lines. These results seemed to indicate at least that the process of curvature adaptation could go on in several parts of the visual field independently. A more direct attack on the question of the limitation of the adaptation to the stimulus area was made in the following experiments.

1. After inspecting or fixating a curved line for 10 minutes, the subject was presented with a set of parallel straight lines drawn side by side filling most of the field of vision. In this situation, S usually reported that the line he was looking at showed the after-curvature, but that the lines right and left of the fixated line seemed straight. The apparent curvature shifted from one line to the other as fixation changed. The results were essentially similar when a bent line was used instead of a curved line.

2. In another procedure, S fixated a short curved line (15 cm) and then looked at a long straight line (100 cm). The report was that on the fixated portion of the straight line a 'bulge' appeared, opposite in curvature to the adaptation line, and that this bulge moved up and down the line as fixation

[19] Experiments with curved lines grouped together into geometrical forms are now being carried out. There is some evidence, as yet unverified, that the amount of after-effect in such arrangements varies with the nature of the form, particularly with its symmetry or asymmetry.

changed. A variation of this experiment was performed using fine lines drawn with a ruling pen instead of heavy lines. For fixation S was given a short fine curved line (5 cm), and for observing the after-effect he was shown a grill of lines set close together (2 mm), covering a relatively large area. The 'bulge' appeared as before, the significant fact being that this distortion seemed to spread very little to right or left, limiting itself to the two or three lines falling within the approximate area of fixation.

3. In still a third experiment S, instead of fixating the center of a curved line, fixated a point 2 cm to the left of it. If now he looked directly at a straight line it appeared straight, but with fixation on a point 2 cm to its left, it appeared definitely curved.

All of these experiments pointed to the conclusion that the negative after-curvature is fairly closely limited to the specific area in which adaptation has occurred. But in order to investigate further this rather important point, and in order to subject it to formal, quantitative test, the apparatus and procedure described in the last section were employed. Even very slight after-effects could be detected and measured when using the adjustable bent line, by requiring S to make the line appear straight and then reading its deviation on the scale.

Two fixation points were used. One was the pinhead at the mid-point of the adjustable line—the 'foveal' fixation point; the other was a small cross on the circular screen, which moved with the adjustments of the line, placed at a fixed distance of 4 cm from the mid-point of the line—the 'peripheral' fixation point. The 4 cm distance represented a visual angle of 5.7°. With this arrangement the line could be fixated either by holding the eyes on the pinhead for 5 minutes (foveal fixation), or by holding them on the cross (peripheral fixation). In turn, the test for after-effect could be made with eyes on the pinhead (foveal test) or on the cross (peripheral test). Both foveal fixation with foveal test and peripheral fixation with peripheral test gave a large after-effect, since the area tested was that in which adaptation had occurred.

Averages for 5 subjects are given in Table IV under the heading 'Same Area Tested.' For all subjects the adaptation and after-effect were somewhat greater when the bent line was exposed peripherally than when it was exposed with its mid-point on the fovea. A similar fact has been reported of the

TABLE IV

NEGATIVE AFTER-EFFECT IN MILLIMETERS OF DISPLACEMENT AT THE CENTER OF A 30 CENTIMETER LINE WHEN A DIFFERENT AREA FROM THAT OCCUPIED BY THE STIMULUS LINE IS TESTED AND WHEN THE SAME AREA IS TESTED

	Different Area Tested		Same Area Tested	
Subject	Foveal Fix. Periph. Test	Periph. Fix. Foveal Test	Foveal Fix. Foveal Test	Periph. Fix. Periph. Test
G	0.93 mm	0.25 mm	2.2 mm	2.8 mm
J	0.65	0.07	3.0	3.7
M	1.60	0.25	4.3	5.1
S	0.87	0.02	2.5	2.6
V	0.65	0.33	3.2	3.7

negative after-effect of movement, in that it is also relatively stronger (*i.e.* more rapid) in the periphery of the field of vision.[20]

The results with which we are primarily concerned, however, are those with foveal fixation and peripheral test and those with peripheral fixation and foveal test. Of these two methods for measuring a possible after-effect when fixation and test area are different, the second is probably the more accurate and reliable since the test is here made with foveal vision and the task of the subject is much easier. The results are given in Table IV under the heading 'Different Area Tested.' If the curvature after-effect were limited strictly to the area occupied by the stimulus line, we should expect to find zero or negligible values. The averages obtained with the second and more reliable method are so low as to be almost negligible. The averages obtained with the first method are higher, especially in the case of subj. M, and it may be that these figures constitute evidence for a very slight 'spread' of the after-effect to the surrounding non-

[20] A. Wohlgemuth, On the after-effect of seen movement, *Brit. J. Psychol. Monog. Suppl.*, 1911, No. 1, p. 70.

stimulated area of the visual field. It is, however, also possible that the figures are higher than they should be because of faulty technique in experimenting. At any rate it is safe to conclude at least that the negative after-effect of curvature in its full strength is limited fairly closely to the specific stimulated area of the visual field.

Series VIII. The Transfer of the After-Effect to the Corresponding Area of the Other Eye

It is an interesting fact that after one has fixated a patch of color with one eye, the negative after-image may be seen with that eye only and does not appear in the corresponding area of the unstimulated eye.[21] It is also interesting to note that, on the other hand, if one looks at a water-fall using only one eye the negative movement after-image does appear vividly in the corresponding area of the other eye.[22] In view of these two facts, it was thought worth while to find out how the curvature after-effect behaves in this respect. Preliminary experiments first demonstrated that curvature is here similar to movement rather than to color—the negative after-effect does transfer in part to the unstimulated eye. S fixated (or inspected) a curved line with one eye, the other eye being either covered with a closely fitting shield or held closed. At the end of the period S shifted to the other eye, and fixated (or inspected) a straight line. Some after-curvature was always reported.

In order to verify and measure this phenomenon of binocular transfer, the apparatus used in two previous experiments was again employed. In one procedure the same eye and the same area that were used for fixation were tested

[21] Under special conditions favorable for binocular rivalry, it may of course seem to appear there. There is, however, some evidence that fatiguing an area of one dark-adapted eye to achromatic light actually lowers the sensitivity of the corresponding area of the other eye. See M. N. Crook, A test of the central factor in visual adaptation, *J. Gen. Psychol.*, 1930, 3, 313–318, and references given therein; also E. Gellhorn, Die Erregungsvorgänge in der Sehrinde des Menschen auf Grund sinnesphysiologischer Experimente, *Zschr. f. d. ges. Neurol. u. Psychiat.*, 1927, **108**, 481–490.

[22] This binocular transfer of the movement after-effect has been noted by several investigators. See for example A. Wohlgemuth, On the after-effect of seen movement, *Brit. J. Psychol. Monog. Suppl.*, 1911, No. 1, p. 28.

for after-effect; in a second the corresponding area of the other eye was tested. In the latter case S fixated the bent line at its mid-point with one eye for 5 minutes. He then closed his eyes while E set the line straight, opened the other eye and, while fixating, reported the appearance of the line. If it seemed bent, he was to adjust it until it was straight. The displacement of the center was then read off by E. The former procedure was identical except that the same eye was used for test. The averages for 3 subjects are given in Table V. Apparently 52 to 82 percent of the negative after-effect transfers to the corresponding area of the other eye.

TABLE V

NEGATIVE AFTER-EFFECT IN MILLIMETERS OF DISPLACEMENT AT THE CENTER OF A 30 CENTIMETER LINE WHEN ONE EYE IS TESTED AND WHEN THE CORRESPONDING AREA OF THE OTHER EYE IS TESTED

Subject	Same Area of One Eye Tested	Corresponding Area of Other Eye Tested	Percent of Transfer
V	3.5	2.1	60%
M	3.2	1.7	56%
J	2.5	1.3	52%
J *	2.7	2.2	82%

* These figures for Subject J apply to transfer from a *peripheral* area of one eye to the corresponding area of the other.

These results with regard to binocular transfer afford an opportunity for some interesting, although perhaps hazardous, deductions as to the locus of the physiological process or processes underlying curvature adaptation. In the first place it is clear that the process cannot very well be exclusively retinal, since in that case adaptation in one eye should have no effect on the vision of the other eye. We must assume that the independent excitations set up by each eye are combined at some point in the chain of visual processes, being thereafter no longer independent. Color adaptation must obviously in large part depend on processes occurring before this point, since the occurrence of this type of adaptation in one eye has no effect on the vision of the other eye. Most of the process involved in curvature adaptation, on the other hand, must take place at or subsequent to this point.

The location of this point of combination is, pending further knowledge of the anatomy and physiology of the visual nervous system, somewhat uncertain. If one could suppose that it is at the visual projection centers in the calcarine region of the occipital cortex, in other words if the right halves of both retinæ are projected in superposition on this region of the right hemisphere, and the left halves in the same region of the left hemisphere, then at least it could be said that curvature adaptation, as a physiological process, occurs wholly or in large part at, or subsequent to, these projection centers, whereas color adaptation occurs before them.

Summary and Tentative Conclusions

It has been shown that when a curved line has been perceived for any considerable length of time, it becomes phenomenally less curved than it was at first, and also that after such a period of fixation or inspection, a straight line appears distorted with an opposite curvature. The adaptation and the negative after-effect are of the same magnitude, the inference being that for the perception process a decrease in curvature to the left is equivalent to an increase in curvature to the right.

In addition to successive contrast in curvature perception, simultaneous contrast also may be demonstrated. The phenomenon is similar to a type of the geometrical optical illusions.

Curvature adaptation and after-effect occur for kinæsthetic as well as for visual perception.

The essential condition for adaptation and after-effect appears not to be merely curvature of a line, but rather departure from rectilinearity. The phenomena appear upon fixating an obtuse angle as well as upon fixating a curved line.

On the whole, the evidence seems to indicate that the negative after-effect of curvature is fairly closely limited to the specific area of the visual field previously occupied by the stimulus line. If a number of curved lines in a group are fixated, adaptation and after-effect can be correspondingly induced in a number of directions simultaneously in different

parts of the visual field. The negative after-effect of curvature shows itself in the corresponding area of the other eye when only one eye has undergone adaptation, the effect, however, being less in the unstimulated than in the stimulated eye.

The adaptation and negative after-effect induced by curvature are similar to those induced by color and brightness in that both are limited to the visual area of the primary stimulus. These effects are dissimilar to those of color and brightness, and similar to those of perceived movement in that they are induced in both eyes by a primary stimulus which is presented to one eye alone. The conclusion may be drawn that the process involved in curvature adaptation, whatever its nature may be, takes place not in the sense organ but at or subsequent to an early stage of the central process.

The above set of facts suggests many lines of speculation and a variety of possible conclusions. But in any investigation of a new phenomenon such as this one, speculations and conclusions are not entirely safe until the phenomenon itself has been verified by other experimenters, and until the details and conditions of its occurrence have been more fully worked out. The present report is by no means complete, and further experimental tests suggest themselves on nearly every page. Those implications of the results, which are here suggested, therefore, are provisional to a more than ordinary degree.

We may first consider the analogy so frequently pointed out between perception of a line on the one hand, and of color (including brightness) on the other. For both types of perception, experiences may be classified into a continuous series with two opposed kinds at the ends and a definite and unique neutral experience in the middle, into which they both merge. The neutral experience may be considered the type or mean of the series. That is to say, for color there is the series blue-gray-yellow or black-gray-white, while for line there is the series convex to right-straight-convex to left. Now in the case of both line and color, the continuous maintenance in

perception of one of the 'end experiences' results in the shift of this experience towards the neutral (adaptation). If now a 'neutral' stimulus is presented, it appears in perception as shifted somewhat towards the opposite 'end experience' (negative after-effect). Furthermore, for both line and color the adaptation and negative after-effect seem to take place only in the area of the visual field correllated with the stimulus object which produced the adaptation. And as a final parallel between line and color, in both cases an area of the neutral experience which is surrounded by an area of one of the end experiences appears slightly shifted towards the other end experience (simultaneous contrast).

On the basis of this set of analogies between line and color perception [23] some rather general inferences are possible. To the extent to which color and line perceptions behave in a similar way, to that extent at least are the perceptual mechanisms similar. And any similarity in the mechanisms underlying two such different kinds of perception is provocative of thought. Color has usually been regarded as 'sensory' while line has frequently been considered 'perceptual.' Color is a 'subjective' experience while line is 'objective.' The former is a 'secondary quality,' the latter is a 'primary quality.' Any interpretation of the facts presented would at least tend to break down rather than to more firmly establish the above distinctions.[24]

The fact of curvature adaptation and after-effect has some bearing on the question of whether or not it is possible to derive an explanation for form perception from the concept of the local sign. Could one start from the hypothesis that every retinal point has a specific spatial value (explaining the origin of the latter in any way one wishes) which differentiates

[23] The writer hopes in a later article, on the grounds of evidence now being collected, to show that the same analogies hold also for perception of visual movement, of direction (vertical and horizontal), and perhaps for certain non-visual perceptions including temperature.

[24] That perception of *line* is a very basic and fundamental kind of experience is convincingly pointed out by James (*Principles of Psychology*, 2, 148–152). Any experience of a brightness or color difference usually carries with it the perception of a line. It may be considered the simplest type, or the elementary unit, of all form perception.

it from all other points, and then ascribe the experience of form, for example a line, to the building up of a unique pattern of local signs, in the course of experience, which in the end comes to mean a line? On this hypothesis, a change in the particular pattern of local signs corresponding to a particular line could be effected only by experience—since experience is the only agent by which the pattern has been made to cohere. The situation to be examined in the light of the foregoing theory is this: a line which at one time appears straight may 10 minutes later appear definitely curved. If the perception of rectilinearity were supposed to be dependent on the arousal of a unique pattern of retinal local signs, which by experience has come to mean 'straight,' then such a phenomenon could not occur unless the local signs constituting the 'straight' pattern had been changed during the 10 minute period. But, from the standpoint of the theory expounded, nothing, under the conditions of the experiment, could have happened during the period which could reasonably be expected to shift the local signs. No conflicting experience from another sense department, for example, has occurred. It is only logical to conclude, therefore, either that some *non-experiential* agent has warped the pattern of local signs, or that the perception of a line cannot possibly be simply and wholly the result of a particular pattern of local signs bound together by experience. In either case, the suggested explanation of form perception fails. The facts seem to point in another direction—towards the hypothesis that perception of a line involves a much more unitary process which has at least some characteristics in common with the process underlying color.

On the other hand, even though the local sign hypothesis is not in itself adequate to explain the facts presented, there is no justification for postulating that perception of line depends upon a configuration in a single homogeneous field of stresses in the cerebral cortex, the configuration being wholly determined by the tendency of the forces within the conducting system to reach an equilibrium, and therefore in part *unpredictable from the stimulus conditions which aroused it.* The situation now to be faced is this: after 10 minutes of fixating a curved line, not

only is the line seen as less curved by a certain amount, but also in this same visual area a straight line appears curved by the same amount in an opposite sense. If the cortical system of stresses is not closely related to the retinal conditions, why, in that case, should the straight line not appear straight?

The fact is that the results of this experiment, at least to the mind of the writer, imply both the lack of an exact point-to-point correspondence between the retinal field and the phenomenal field, and the existence of such a correspondence, each being true for a particular set of perceptual conditions. They imply, in other words, both that the peripheral (retinal) processes do not determine the central (phenomenal) processes, and at the same time that they do closely determine them; that the structural connections between the two are not precise and that they are precise. The fact of curvature adaptation means that a retinal curved line may come to be represented in consciousness as a phenomenal less-curved line. The inference, surely, is that a specific point-to-point correlation does not hold in these circumstances. On the other hand the phenomenon of negative after-effect means that when a retinal curved line is correllated with a phenomenal less-curved line, then for the area in question a retinal straight line is correllated with a phenomenal oppositely-curved line. The inference is that a point-to-point correspondence between the retinal and the phenomenal field does at this time exist—a correspondence which has been shifted from the normal one but which is nevertheless specific. The conclusion from these two propositions taken together would seem to be that there is a system of point-to-point correllations between retinal field and phenomenal field but that it is not a rigid system. So far from being rigid, the relations of correspondence are subordinate to the process of curvature adaptation, and, by the activity of this process, whole sets of point-to-point correlations are changed together. It may be inferred that the perception of a line as such cannot depend upon, or arise from, the fact of a point-to-point correspondence, since the point-to-point relations are under some circumstances modified by the perception of a line.

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SOME CONFIGURATIONAL PROPERTIES OF SHORT MUSICAL MELODIES

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For some time the senior author had observed a curious phenomenon while listening to the Seashore test of tonal memory. In this test series of short melodies are played, each melody being repeated immediately with one tone changed. The subject is instructed to state which one of the tones is altered in the repetition. The curious phenomenon to which we refer is that very frequently not only one tone, but several tones may appear to be altered. Other observers have been asked to look for this effect, and almost without exception casual observations seemed to agree that more than one tone in a melody may be distinctly changed.

This phenomenon appeared to be all the more significant in the light of the now well known Gestalt theories. We are frequently reminded that "the whole is more than the sum of its parts," [1] and that a member of a configuration possesses properties which it owes to the total in which it appears. Each tone in a melody, according to these principles, is subject to certain dynamic influences, due to the conditions under which it occurs. Whether these dynamic influences upon any single tone are to be regarded as independent, measurable, inter-tonal stresses, or whether they are to be thought of as a single resultant effect due to the total 'shape' of the melody, is a question upon which not all of us would agree.[2] This question is not a vital one in the present study, however, and so it will not be discussed at length here.

Another frequent assertion about the nature of a Gestalt

[1] Helson, H., The psychology of Gestalt, *Amer. J. Psychol.*, 1925, **36**, 347, 360.

[2] When the writers use the terms 'dynamic influences' and 'stresses,' and the like, they refer to hypothetical electro-chemical processes in the brain or other nervous structures that may be involved.

may be paraphrased to read, "changing one member changes all the rest." [3] This property of a configuration applies more directly to the phenomenon in question. The Seashore melodies seemed to us to provide very apt examples which might prove or disprove the last mentioned principle, at least as applied to melodies. No one, as far as we knew, had attempted to test this principle by direct experiment in the field of melodies. The principle appears to have been taken for granted by the configurationists, without any more factual basis than a few scattered observations. The results of the present study not only seem to bear out the principle completely but also to indicate what some of the dynamic properties of a melody are. We believe that the way is opened to a rather direct experimental attack upon the inner properties of a configuration such as a melody.

After some preliminary considerations, it was decided to use the Seashore tests for the stimulus material. These melodies are admittedly not the very best for the purpose; on the other hand they have many saving virtues. The tones are produced by a flute, which guarantees a relatively true pitch and fairly good tonality. No equipment was available for making a registration of the sound waves in order to see whether or not the unaltered tones were faithfully duplicated when the melodies were repeated. This would have been desirable, but the reader will probably be convinced from the results that this unknown factor is not fatal to the conclusions. The fact that a phonograph record was used guarantees that the stimuli were exactly duplicated from one time to another. The same type of phonograph needle was provided throughout the experiments. The speed of the phonograph was set at seventy-eight revolutions per minute, as in giving the Seashore tests.

The arrangement and variations in the kinds of melodies were fairly satisfactory for the purpose. The melodies range from two to six tones in length, with ten melodies of each length. While they are arranged in groups of ten, and the order of the melodies within the group cannot be changed

[3] See Helson, *Op. cit.*, p. 348.

from one presentation to another, it is possible to vary the order in which the groups are given. The altered tones are made both higher and lower in pitch. Although the number of raised and lowered tones is not equal in every group, in the total set of fifty melodies, the altered tone is made higher in twenty-seven cases and lower in twenty-three cases. Within each group, the altered tone appears about equally often at different positions in the melody. There is a fairly good distribution of them in this respect. The melodies may be classified as to 'shape' into five classes: (1) upward slant, (2) downward slant, (3) V-shaped, (4) inverted V-shaped, and (5) zig-zag. These terms indicate the trends in pitch. The melodies are not evenly distributed among these five classes. The number of examples are, 5, 7, 12, 9, and 17, in classes 1 to 5 respectively. Naturally the two-toned melodies could have only an upward or downward slope. And as might be expected, the longer melodies tend away from classes 1 and 2 and toward class 5. These factors should be balanced more evenly in an ideal set of stimuli. We had to assume in these experiments that any factor not under control varied in a rather haphazard or chance manner.

The procedure for judging the changes was worked out somewhat on the order of the method of constant stimulus differences. During the first playing of the record, the *O*s were instructed to listen to the melody *as a whole*—this was emphasized—but they were to report upon only the *first* tone of each melody. If in the repeated melody this tone seemed higher, *O* judged it 'plus,' and recorded a plus sign on his record, if lower he recorded a minus sign, and if it seemed the same he recorded an 'equal.' In the next playing of the record, the *O*s judged the second tone in each melody, and so on until the sixth tones in the six-tone melodies were all judged. This made a total of 200 judgments which were obtained in a single hour of observation. The order in which *O* judged the first, second, third, etc. tones was changed from day to day. This change, together with the variation in the order of the groups, helped to break up habits of judgment that otherwise might have formed.

Some objection might be raised because of the fact that the *O*s were asked to report upon only one tone at a time. Would this not tend to break up the total Gestalt? As a matter of fact, the *O*s very soon fell into an attitude that seems perfectly satisfactory, an attitude of hearing the melody as a whole, as they were instructed to do, but in being able more or less automatically to report upon any specified tone in the melodies. Their reports upon attitude indicate, further, that it is almost impossible to isolate a tone from its melody by a mental set; the rest of the melody forces itself upon one. After most of the observations were completed, a new type of judgment was attempted, in which *O* did try to report upon the whole melody at once. The *O*s were given a pencil and coördinate paper. They were told that upon the first hearing of the melody they were to diagram the rise and fall of the tones very quickly, and after the second hearing of the same melody they were to superimpose upon this diagram a similar drawing of the rise and fall of the tones the second time as compared with the first. The judgments were still comparative, but *O* had to report upon the several members at once. This proved rather difficult at first, but with a little practice it could be done with some degree of assurance. The greatest difficulty was that the phonograph record did not allow enough time between melodies for these drawings. The few results obtained in this manner were in every respect like those obtained under the first instruction and the *O*s reported that they could see no essential difference between the two methods, except that the second was more difficult to execute. More intensive work with the second method might, however, reveal some significant differences. From our limited results with the second method, though, we would not expect any very great differences.

Seven *O*s including the two authors, two graduate students in psychology, and three undergraduate majors, two of whom had had a course in advanced experimental psychology, took part in the experiments. Three of the *O*s had had considerable training in music, two others had had a limited

amount of training, and two had had no training at all. At the beginning of the experiments and again near the middle of them, tests of discrimination of pitch were given to all *O*s. For this purpose the Seashore test of sense of pitch was employed, using the B side of the record. The differences in pitch represented on this side of the record are .5, 1, 2, 3, 5, d.v./sec. Each difference is repeated ten times in one playing of the record. Altogether, the *O*s listened to six playings of the record, giving sixty judgments upon each stimulus-difference. The judgments were simply 'higher' or 'lower' as in the Seashore test. The results were treated by the method of right and wrong cases, and a limen was computed for each *O*. These limens were rather near together in value, as one can see from the following seven: 1.42, 1.70, 1.94, 2.16, 2.32, 2.46, and 3.13. While the best *O* makes discriminations about twice as fine as the poorest, this probably by no means approaches the entire range of ability in discrimination of pitch. There was no indication that any of the *O*s improved significantly in discriminating pitch during the first half of the experiments. The preliminary tests of discrimination and the later ones gave essentially the same results.

In judging the melodies, every *O* observed and reported upon each of the 200 tones involved during each experimental hour. The *O*s were given the experiments in a group, with three observation hours a week, at a late afternoon hour when distracting sounds were at a minimum in the laboratory. The room was not sound proof. Every *O* devoted twenty-five hours of observation to the study, which resulted in a total of 175 judgments upon each of the 200 tones, or 35,000 judgments in all.

During each hour's observations the *O*s were asked to write down anything of an introspective nature they wished. The instructions for this were very general. They were merely encouraged to record anything that they considered of importance. All of them either knew the nature of the problem beforehand, or guessed it before many sessions had been held. They were requested not to discuss their opinions with one another. A comparison of all the introspections

which were obtained revealed certain uniformities of opinion as to the effect of the altered tone in the melody. The more common opinions were used as guides for the treatment of the quantitative data. We had no *à priori* notions as to the kind of dynamic effect the altered tone should have, except that the effect might be stronger in the proximity of the altered tone. But we did not know whether to expect a shift of the neighboring tones in the same direction or in the opposite direction, or whether both changes might occur at different times. The most common opinions, and the ones which we took seriously, were as follows:

1. *O* was frequently sure that a tone had changed, but was uncertain as to the direction of the change. This was truer of the longer melodies than of the shorter ones.

2. *O* was surer of his judgments of the first and last tones in a melody than of the intermediate tones.

3. *O* noted some tendency for the first tone of a melody to appear higher when the melody was repeated, and a stronger tendency for the last tone to seem lower the second time it was played.

4. The altered tone seemed to raise or lower the whole melody along with it. At times this effect appeared as a shift from a minor to a major key or vice versa.

No attempt will be made to interpret these opinions until we see whether or not the objective data verify them. They do agree with the quantitative results in many respects, but the latter disclose some additional facts that were overlooked by the introspections of the *O*s. We shall consider the four points mentioned above, one at a time.

Stability of the Tones

If the unaltered tones (U-tones) really appeared to be unchanged in the two playings of each melody, they would naturally be judged 'equal' in a great majority of the experiments. If they appeared to be changed, there would be a greater number of plus and minus judgments, although the proportions of the plus and minus judgments will vary as to the degree of certainty of the direction of the change. The

percentage of 'equal' judgments may be taken, therefore, as an indicator of the stability of the different tones. By 'stability' is meant their agreement with the stimulus relations. Not that the phenomenal pitch of a tone and its repetition shall correspond exactly with the frequency of the tone-stimulus. This would involve us in a constancy hypothesis which is irrelevant to the issues. We mean that a U-tone is stable when there is phenomenal equality corresponding to a physical equality, and there is a phenomenal change in a certain direction corresponding to a physical change in the same direction, as in the case of the altered tones (A-tones).

We are going to assume, then, in the case of the U-tones, that the higher the proportion of equal judgments, the more stable the tone. If a U-tone is unstable, we are not concerned with the direction of the instability, that is with the greater or less proportion of plus or minus judgments that it received. It might be that the number of 'not-equal' judgments would increase automatically along with the preponderance of pluses or minuses among them; that the *O*s would not judge 'plus' or 'minus' unless they were reasonably sure of such a judgment and that whenever they were in doubt, they would judge 'equal.' But they were instructed to guess 'plus' or 'minus' whenever they detected a change. This instruction was intended to bring out as many 'not-equal' judgments as possible for the sake of the reliability of the proportions of 'plus' judgments for each tone. The instruction also served to confine the 'equal' judgments to those cases that were actually phenomenally equal, and hence to make the number or percentage of 'equal' judgments a measure of stability. In order to test this matter more accurately, we made a study of the correlation between the proportion of 'equal' judgments and the proportion of 'plus' judgments among the 'not-equal' judgments. Or rather, the 'not-equal' judgments were grouped according to their distance from a proportion of 0.50. If the *O*s had tended to throw all the doubtful cases, in which they could detect a change but not its direction, with the 'equals,' the correlation should be negative, for the farther

away from 0.50 the proportion of 'plus' judgments is, the fewer 'equal' judgments there would be. With 150 such pairs of values correlated, the coefficient approached zero so closely that it was insignificant. This indicates that the proportions of equal judgments may be used as relative measurements of the stability of the U-tones.

In this section, as well as in all that are to follow, the judgments of the seven *O*s were pooled. This was necessary in order to have a large enough number of judgments (175) on each tone. Inspection of the data showed that the directions which the judgments of the seven *O*s took for each tone were very similar. The differential limens of the seven *O*s were close together. By pooling the judgments, personal biases concerning the kinds of changes expected were also minimized. Only the more objective, impersonal, phenomenal changes would survive the pooling, unless the seven *O*s were subject to the same biases. There is no reason to believe that the latter is true.

The relation between the length of melody and the average stability of the tones is given in Table 1.[4] In this table

TABLE 1

STABILITY OF THE TONES AS A FUNCTION OF THE LENGTH OF THE MELODY

Length of Melody	Mean Proportion of 'Equal' Judgments for the U-Tones	N	Mean Proportion of Correct Judgments for the A-Tones	N
2	.759	10	.825	10
3	.622	20	.723	10
4	.620	30	.697	10
5	.598	40	.595	10
6	.586	50	.572	10

we find a continuous decrease in the proportion of 'equal' judgments as the melody becomes longer. This may be due to a number of things. In the first place, it may be caused by the increasing dynamic interplay between the tones in the melody as one tone is changed. One might expect, however,

[4] Complete tables giving the results by individual *O*s and the numbers of 'plus,' 'minus,' and 'equal' judgments for each of the 200 tones are not published in order to save space. Only pertinent averages are given here. The complete record of the results may be found in a Master's thesis by Ruth A. Hilton in the library of the University of Nebraska.

that the longer the melody, the less disturbance there would be for each of the unaltered tones; they might serve to stabilize one another. In the second place, as the melodies increase in length, more tones are interpolated between the two that are to be judged, and more time elapses between the two. The latter factor, the increase in time interval, probably is not nearly as serious as the former. It has been shown that an interpolated stimulus between pairs of lifted weights does disturb discrimination.[5] It makes discrimination poorer, and if the interpolated weight is heavier or lighter than the pair to be compared, it shifts the phenomenal value of the second weight usually in the opposite direction, producing a kind of contrast effect. Now if the pitch of tones is affected in a similar manner, several interpolated tones would tend to make the impression of the second comparison tone rather uncertain. This should make the tones in the longer melodies less stable than those in the shorter ones. Apparently this is what happens.

The combined effect of interpolated tones and lengthened time interval should have a similar effect upon the tones that are physically altered (A-tones). All these tones were altered to the extent of a semitone. The accuracy of the judgments upon these tones for each length of melody was measured by the proportion of correct judgments, either 'plus' or 'minus.' The averages of the proportions for the groups of melodies appear in the fourth column of Table 1. The decrease in accuracy is again continuous as the melodies become longer. From the total range of proportions in this column as compared with those in column two, it appears that the decrease in stability of the A-tones is greater than that for the U-tones. If this is a fair comparison, we can conclude that the interpolation of an increasing number of tones interferes more with the judgment of a difference than with a judgment of equality. This is more strikingly seen by comparing the two-tone melodies with the six-tone melodies. In the former it was easier to judge differences correctly; in the latter it was easier to judge equalities correctly.

[5] Guilford, J. P., and Park, D. G., The effect of an interpolated weight upon psychophysical judgments, *Amer. J. Psychol.*, 1931, **43**, 589–599.

The relation between the stability of the U-tones and the position in the melody is indicated in Table 2. The

TABLE 2

STABILITY OF THE U-TONES AS A FUNCTION OF POSITION WITHIN THE MELODY

Length of Melody	Positions in the Melody						
	1	2	3	4	5	6	N
2	.695	.801					4–6
3	.667	.591	.613				6–7
4	.675	.565	.617	.610			7–8
5	.646	.625	.571	.509	.640		8
6	.634	.660	.651	.557	.545	.489	8–9

tendencies that look promising in this table are two: (1) A higher stability at the beginning of a melody than at the end, and (2) a higher stability at the ends than in the middle of the melodies. In order to check this more rigidly, we have combined all end values and all middle, or intermediate values in Table 2 and have found averages. Since the length of the melody is also a factor in determining stability, we have used only three mean values for melodies of each length, a mean for the first, intermediate, and last positions. Each length of melody is therefore weighted equally in the final averages for the three positions. The two-tone melodies were entirely eliminated here, because they contain no intermediate tones. In the six-tone melodies, we have included positions 2 and 5 as end-tones since they are relatively more like terminal tones than middle tones. The same procedure was applied to the A-tones. The results are given in Table 3. No P.E.s or critical ratios were computed, since

TABLE 3

MEAN VALUES INDICATING RELATIVE STABILITY OF END-TONES AND MIDDLE-TONES

	U-Tones			A-Tones		
	First	Middle	Last	First	Middle	Last
Mean proportion.....	.659	.588	.595	.764	.776	.837
N.....	38	61	41	12	19	9

they would be of uncertain value and meaning here. N refers to the total number of cases from which the means were derived, although, as was said before, the single cases (tones) were not weighted equally.

Table 3 seems to follow Table 2 in regard to the U-tones. The order of stability is highest for the first tones, lowest for the middle tones, and the last tones are only a little more stable than the middle ones. As for the A-tones, they present quite different results. The last tones are apparently the most stable, and the first ones the least. This may be a real difference between the A-tones and the U-tones. We have further verification for this difference.

It has been possible to test this difference with other data. The Seashore test of musical memory had been given as a regular part of the elementary laboratory course in the same manner for about ten years. The results of these tests were carefully inspected. All those record sheets which seemed to show that the subject had followed directions faithfully were used. There were 412 of these records. In this test, as the reader is probably well aware, the subject makes only one judgment upon each melody. He is to state which one of the tones has been changed, without stating further what the direction of the change is. We tabulated the number of times each one of the 200 tones was judged as changed by the 412 subjects. The relative stability of each A-tone can be measured by the number of times it was reported changed. The number of times any given U-tone was reported changed, on the other hand, measures the relative instability of that tone. Table 4, which corresponds to Table 1, shows how the stability of the tones varies with the length of the melody.

Column 3 of Table 4 shows the dropping off of the number of correct judgments as the melodies become longer. Of the 412 judgments, 94.4 percent are correctly placed in the two-tone melodies, but only 42.8 percent, or less than half, in the six-tone melodies. Column 2 shows the increase in the number of incorrect judgments for the average U-tone as the melodies become longer. If the lengthening of the melodies had no effect, we would expect the number of incorrect

TABLE 4

RELATIVE STABILITY OF THE A-TONES AND U-TONES AND THE LENGTH OF MELODY

Length of Melody	U-Tones	A-Tones	
	Average Number of Incorrect Judgments	Average Proportion of Correct Judgments	Average Number of Incorrect Judgments
2	22.0	.944	22
3	27.8	.824	56
4	26.0	.813	76
5	37.6	.615	158
6	48.3	.428	236

judgments here to remain constant. The laws of chance would demand that. But apparently the dice are loaded, and in the six-tone melodies more than ten times as many judgments are given to the five U-tones than are given to the one U-tone in the two-tone melodies. Incidentally, this argues against weighting each correct response equally, regardless of length of melody, in scoring the Seashore test.

The relation found between stability and position in the melody finds substantiation in Tables 5 and 6, which correspond to Tables 2 and 3 respectively. The number of cases upon which the values depend are the same as in Tables 2 and 3; and therefore the N-values are not repeated here.

TABLE 5

INSTABILITY OF THE U-TONES AND POSITION IN THE MELODY, MEASURED BY THE NUMBER OF INCORRECT JUDGMENTS

Length of Melody	Position					
	1	2	3	4	5	6
2	13.3	29.5				
3	22.3	33.7	26.6			
4	21.0	45.8	17.0	25.3		
5	37.5	41.1	40.8	43.1	25.4	
6	32.4	46.6	62.7	45.1	36.2	65.9

TABLE 6

INSTABILITY OF THE U-TONES AND A-TONES IN DIFFERENT POSITIONS, IN TERMS OF MEANS OF INCORRECT JUDGMENTS

	First	Middle	Last
U-tones	30.1	39.9	32.1
A-tones	140.0	128.2	121.2

The similarity of these tables to Tables 2 and 3 is very striking. For each length of melody, and for melodies regardless of length, the U-tones are less subject to phenomenal change at the beginning than at the end and at the two ends than in the middle of the melody. The A-tones, as before, show a decreasing instability from the beginning to the end of the melody. If the results for the A-tones agreed with those for the U-tones, we might attribute the phenomenon to the 'law of precision.' The end tones, if this were true, would seem to serve as anchorage points for the melodies. In view of the difference between the U-tones and the A-tones in this respect, this law cannot be said to apply generally. It would seem to apply only to the U-tones.

On the whole, the conditions are a little too complicated to give a clear test of stability of the various tones. A much simpler arrangement of conditions, such as the comparison of a tone in the melody with a single tone outside the melody, would have given a much surer answer to the question of stability. We have investigated the matter with our data only because our *O*s repeatedly reported that tones, especially in the longer melodies, were different in pitch during the repetition of the melody, but that the direction of the change was more uncertain, and that the end tones were easier to judge than the middle ones. It may be that a distinction should be made between 'easy to judge' and stability. But everywhere else in psychological studies, the degree of certainty of judgments is positively and significantly correlated with correctness of judgment. Granting this argument and accepting the results of this section of the paper, we are brought to three tentative conclusions: (1) That the longer the melody the less stable the tones; (2) That the first, and perhaps the last U-tones of a melody are more stable than the intermediate ones; and (3) That the A-tones seem to be progressively more stable, *i.e.* more correctly judged, from the beginning to the end of the melody. These possibilities should at least lead to further investigation.

Rising and Falling of First and Last Tones

In general the *O*s agreed that the first tones seemed higher and the last tones seemed lower on the average in the second playing of the melodies. How well do the objective results bear out these opinions?

In this and in following parts of the paper, a new method of evaluating the psychological changes in the tones was employed. Only the 'greater' and 'less' judgments were used. For each of the 200 tones, A-tones and U-tones alike, the proportions of the 'plus' judgments among the not-equal judgments were computed. It is true that equal judgments were allowed and that they were given in great numbers. A rather erroneous technique sometimes used in the past has been to divide the equal judgments equally among the greater and the less. This can no longer be countenanced, since the intensive work of Brown on the use of equality judgments.[6] Brown proved conclusively that with extremely small differences, when the *O* is forced to judge only 'greater' or 'less,' he is right more often than he is wrong. We think, therefore, that the equal judgments should be divided in the same proportion as the actually given 'greater' or 'less' judgments, which is equivalent to ignoring them entirely. This is what has been done here.

The next question that arises concerns the reliability of these proportions. As everywhere else, this is measured by their probable errors and the probable error of a proportion is an inverse function of the square root of N, the number of judgments. Among the 200 tones, the number of plus and minus judgments combined differed considerably. Obviously, the number was usually greater among the A-tones than among the U-tones. Among the latter, N varied from 15 to 150, out of a possible 175, with a mean of 70. The largest probable error when N is 70 is .035. This is when the proportion is exactly .500. As the proportions approach 1.00 or 0.00, the probable error approaches 0.000. If we can assume that if the *O*s had been forced to eliminate the equality

[6] Brown, W., The judgment of difference, *U. of Cal. Pub. in Psychol.*, 1910, 1. No. 1.

judgments, the proportions would have remained the same, then N could be taken as 175 and the reliabilities of the proportions would be considerably higher than they are. We have not taken the liberty to make this assumption. We have even eliminated from consideration all those cases in which N was less than 30, or in which N was less than 35 and the proportion so near .500 that there were many favorable chances for the proportion to change to the opposite side of .500, should the experiments be repeated. There were but six such cases. The remaining 144 U-tones may be said to have proportions reliable enough for our consideration.

From the proportions of 'greater' judgments, scale values were found for the tones by reference to Cattell's Table.[7] These values use the P.E. of the normal probability curve as their unit of measurement. Proportions above .500 give positive scale values and those below .500 give negative. The former indicate a shift in pitch upward and the latter a shift downward, and the scale values represent distances on a psychological continuum. We are assuming in what follows that these are real psychological values or measurements and that they may be treated as such.[8] The most questionable step in the whole procedure, it seems to us, is in pooling the judgments of the seven *O*s. This should merely serve, however, to affect the size of the psychological unit of measurement, and hence it is not serious. The conclusions which follow would not be different had we used the original proportions instead of the scale values.

Using only the U-tones, the average changes in the first and last tones were computed for the melodies of each length and for all melodies combined. The results are given in Table 7.

One might conclude from these results that there is some slight tendency for the first tones to sound higher and the last tones to sound lower. But this is not at all certain,

[7] Cattell, J. McK., and Fullerton, K. S., On the perception of small differences, *Pub. of the U. of Penn., Phil. Series*, No. 2, 1892.

[8] For a justification of this procedure see an article by the senior author entitled 'Some empirical tests of the method of paired comparisons,' *J. Gen. Psychol.*, 1931, 5, 64–76.

TABLE 7

AVERAGE CHANGES IN THE U-TONES IN THE FIRST AND LAST POSITIONS IN THE MELODIES

Length of Melody	Average Change			
	First	N	Last	N
2	1.340	4	.460	4
3	.357	6	.193	6
4	−.279	8	−.101	8
5	.286	6	−.157	8
6	−.037	8	−.224	9
Weighted means	.215	32	−.031	35
P.E.$_{av.}$	.146		.129	
Diff.		.246		
P.E.$_{diff.}$		.195		

although the difference does seem rather consistent for melodies of different lengths. The small number of cases does not permit a definite conclusion on this point. It is worth pointing out, however, that this tendency holds in spite of the fact that the final tones are actually higher in pitch than the first ones in twenty-six of the fifty melodies. It cannot be said, therefore, that this tendency is due to a prevailing downward slope of the melodies.

AVERAGE CHANGE IN U-TONES AS COMPARED WITH CORRESPONDING CHANGE IN A-TONES

We now come to the main problem. What is the effect of the change in the A-tones upon the U-tones? Tables 8 and 9 provide us with conclusive evidence. There are strong dynamic effects produced upon the U-tones by the actually changed tones.

TABLE 8

PSYCHOLOGICAL CHANGES IN THE A-TONES

Length of Melody	When A-Tone is Higher	N	When A-Tone is Lower	N	Weighted Absolute Means
2	2.515	7	−2.668	3	2.561
3	1.405	4	−2.014	6	1.771
4	2.898	5	−1.933	5	2.415
5	1.311	8	−1.596	2	1.368
6	1.208	3	−1.160	7	1.174
Weighted means	1.919	27	−1.785	23	1.858
P.E.$_{av.}$	.260		.111		.224

From Tables 8 and 9 several conclusions may be drawn. The A-tones are changed about five times as much on the average as the U-tones. The U-tones in every length of melody with one exception shift in the same direction, on the average, as the A-tones. The exception is in the six-tone

TABLE 9

PSYCHOLOGICAL CHANGES IN THE U-TONES

Length of Melody	When A-Tone is Higher	N	When A-Tone is Lower	N	Weighted Means
2	.963	5	−.527	3	.800
3	1.027	8	−.429	11	.681
4	.438	14	−.697	15	.572
5	.355	31	−.351	7	.363
6	.121	15	.027	34	.046
Weighted means.....	.508	63	−.264	70	.353
P.E.$_{av}$.....	.088		.086		.057

melodies when the A-tones were lower, and here the average change was very close to zero. In both types of tones, the amount of change decreases as the melodies increase in length. Apparently this occurs at about the same rate, so it cannot be said that the effect of the change of one tone in a melody becomes less and less effective as its influence is spread over a wider range of neighbors. This factor is an unknown, largely because of the technique employed. Judgments of both A-tones and U-tones become less and less certain as the melodies increase in length due to the interpolation of additional tones and longer time intervals between the pairs to be judged.

On the whole, the certainty of the changes in the A-tones was much greater than that for the U-tones, although the changes in both were statistically reliable. The average changes in the U-tones were from three to five times their P.E.s, whereas the average changes in the A-tones were from seven to sixteen times their P.E.s. It should be pointed out that in many specific instances, the U-tones were changed psychologically more than the A-tones. One should also remember that the A-tones themselves may be subject to dynamic effects. This was most strikingly shown in three

cases where the A-tones were changed phenomenally in a direction opposite to the physical change, and in a few more cases where the A-tone was psychologically unaltered. The effects are very intricate and complicated indeed. Some of them are undoubtedly due to the alteration of the A-tones, but some of them might occur without any such alteration at all. If the work were to be repeated, one should by all means use 'control' melodies, with no A-tones at all. For example, the slight tendency for first tones to rise and for last tones to fall might occur independently of any A-tones whatever. In fact, in our limited set of melodies, this tendency might have been obscured by the presence of the A-tones and the fact that they were not distributed purely by chance throughout the melodies used.

Changes Before and After the A-Tone

The above facts seem to prove that an alteration of a single tone upward or downward does tend to shift the melody as a whole up or down with it. A further analysis of this wholesale effect is possible. Is the effect stronger before or after the A-tone? And if there is any such difference, will it make any difference whether the A-tone be raised or lowered in pitch? Table 10 shows such an analysis.

TABLE 10

Phenomenal Changes Before and After the A-Tone

Length of Melody	Before A-Tone				After A-Tone			
	A-Tone Higher	N	A-Tone Lower	N	A-Tone Higher	N	A-Tone Lower	N
2	1.127	3	−2.006	1	.731	2	.212	2
3	1.189	3	− .265	6	.930	5	−.625	5
4	.877	4	− .950	10	.263	10	−.192	5
5	.568	15	.274	3	.156	16	−.871	4
6	−.095	9	.084	11	.446	6	.000	23
Weighted means	.532	34	− .366	31	.480	29	−.184	39
P.E.$_{av.}$	.124		.188		.102		.108	

The surprising result seems to be that on the whole the dynamic effect is exerted more strongly before the A-tones than after them. It is surprising enough to find any influence

at all upon the tones preceding the alteration; to find that the retroactive effect is perhaps even stronger than the after-effect is even more astonishing. Perhaps the difference between the strength of the effect before and after is not sufficient to demand an explanation. But certainly, the retroactive effect is significant enough to demand a good reason. The average changes before the A-tones are at least four times their P.E.s and are therefore reasonably significant.

This retroactive effect is very likely a result of memory for the melodies and the anticipation of the change in the A-tones before they appear during the second playing. Introspections tend to show that this is true. While the memory for the melodies was never very complete or detailed, there developed a kind of feeling for the change that was to come during the repetition of the melody. The anticipation was often more unconscious than conscious, because of this lack of a detailed memory for the tones in the melodies. It is not unreasonable, therefore, to put forth the hypothesis that the retroactive effect was due to the activity, conscious or unconscious, of a memory trace, occurring before the A-tones came.

We would also like to venture another hypothesis to explain the smaller effect upon the tones following the A-tones. This theory is that the tones following are more subject to a kind of contrast or compensatory effect. It is based first upon the rather frequent report from some of the *O*s that the tones following the A-tones were carried along in the same direction as the A-tones, unless they were very much higher or lower than the A-tones, in which case they were altered in the opposite direction. It is based, secondly, upon the results of Guilford and Park in a study of lifted weights.[9] It was found that when a weight follows a decidedly lighter weight, it tends to be judged heavier, and when it follows a much heavier weight, it is usually judged lighter. In these experiments with weights, although the compensatory effect was by far the more common, under certain conditions the weight that was judged would be influenced in the *same* direction as the immediately preceding weight. In the case of

[9] Guilford, J. P., and Park, D. G., *Op. cit.*

the melodies it seems that the compensatory effect is the less common. Further experiments will be needed in order to determine under what conditions it is 'assimilatory,' that is, in the same direction as the preceding tone.

Proximity of the Tones and the Dynamic Effect

One would naturally suppose that since there is a dynamic effect of one tone upon another, the nearer a tone is to the altered one, the greater the effect would be. It is an easy matter to test this supposition with these data. Since we have already found some indication that tones before the A-tone are influenced more than those after it, and that this fact differs when the A-tone is raised or lowered, we shall also consider the factor of proximity along with the before-and-after relationship. The degree of proximity varies from one to five tones away from the A-tone, but there were only a few scattered cases in which they were four and five tones distant. Therefore we have combined the last two into a single class. The results appear in Table 11.

TABLE 11

Proximity and Dynamic Effect

Degree of Proximity	Before the A-Tone				After the A-Tone				Before or After the A-Tone			
	A-Tone Plus	N	A-Tone Minus	N	A-Tone Plus	N	A-Tone Minus	N	A-Tone Plus	N	A-Tone Minus	N
1	.724	19	−.336	15	.623	20	−.079	15	.647	39	−.207	30
2	.163	8	−.415	9	.181	10	−.174	10	.173	18	−.288	19
3	.542	5	−.707	5	.019	7	−.424	6	.237	12	−.552	11
4–5	.159	2	.255	2	−.186	1	−.187	7	.044	3	−.089	9

A few generalizations may be made tentatively from this table. When the A-tone is raised, there is a general decreasing effect, the greater the distance of the U-tone from it. This decrease is not perfectly regular and continuous, especially before the A-tone, but with larger numbers of cases it might conceivably become so. When the A-tone is lowered, there is a continuous *increase* in the dynamic effect with decreasing proximity, for the first three degrees of proximity, with a sudden lapse in this rule in the fourth and fifth positions

removed from the A-tone. This rule holds both before and after the A-tone, and so it may be given some credence. Just why the results should be so different for the two cases, when the A-tone is raised and when it is lowered, we do not attempt to explain. In the one case it is as we had expected, and in the other it is just the opposite. In both there is some tendency for a maximum effect in the third tone distant. Once more, the complicated nature of the dynamic influences is clearly demonstrated.

Shape of the Melody and Dynamic Effects

Does the shape of the melody and the position of the altered tone in that shape have anything to do with the resulting readjustments of the other tones? In order to see such effects more clearly, we resorted to graphic representation of the melodies, together with the actual changes and all the phenomenal changes. On cross section paper, allowing one square to each semitone, we plotted the rise and fall of the melodies. The alteration was indicated by dotted lines superimposed upon the other lines. The phenomenal changes were then inserted in red. In order to translate the degree of phenomenal change into semitone units, we found from the average change in the 50 A-tones that a semitone was equivalent to 1.858 P.E. units, or the units of the psychological scale which we were using. This enabled us to transmute the phenomenal changes, roughly, into the units of the musical scale. There are obvious errors in this procedure, for the degree of phenomenal change in the A-tones varied from one length of melody to another as is shown in Table 8. But the analysis which we are making in this section is more qualitative than quantitative, and the approximations which were made enabled us to see certain effects that might be attributed to shape alone.

One outstanding effect was noted in connection with the V-shaped melodies, including the inverted V-shaped ones as well as the upright ones. If the A-tone came at the point of the V and tended to lessen the sharpness of the V, the A-tone changed little or none at all in five of eight cases, and in the

other three cases the change was very slight, not over half a semitone as measured according to our method. The end-tones, however, tended to change in the same direction as the actual change, with the result that the shape of the V was sharpened after all. Quite in contrast with these cases were the V-shaped melodies in which the A-tone changed so as actually to sharpen the V. In four cases of five, the A-tone was decidedly changed and the end tones changed very little. The fifth case presented a less striking example of the same thing. The law of 'emphasis' might have been at work here, and a sharpening of the V-shaped melodies may have occurred without any alterations of the tone at the point. Mere repetition of the configuration may have accentuated the outstanding feature of the melody. At this point again, some control experiments are needed without the use of altered tones.

There were not enough examples of the other shapes of melodies to warrant the search for uniform changes. Undoubtedly the problem of shape is important, with and without the alteration of an A-tone. In different shapes there might be certain anchorage points about which the melody hinges. Certain tones in a shape would be found to be more stable than others, more resistant to change, as we found in the V-shaped melody. Alterations in one direction may be resisted more than in the opposite direction, due to the position of the tone within the shape. The two or three cases in our data in which the A-tone changed phenomenally in the opposite direction to its physical change surely demand an explanation. It cannot be a matter of chance.

Summary and Conclusions

In the Seashore test of musical memory in which melodies varying in length from two to six tones are played twice in succession with one tone altered in the repetition, it is apparent that other tones than the one actually altered may also change in pitch. Seven subjects were asked to judge the 200 tones involved in these melodies as to whether they were higher, lower, or the same in the repetition, a total of 25 times

each. From a compilation of the results, some conclusions have been reached concerning the dynamic relationships of tones in a melody:

1. The tones, altered or unaltered, seem to be rendered unstable by the presence of the other tones in the melody. The longer the melody, the more unstable they become. The unaltered tones are more stable at the beginning than at the end of the melody, and at either end than they are in the middle. The altered tones are progressively more stable from the beginning to the end of the melody. By stability is meant a close correspondence between physical and phenomenal fact.

2. Introspections and objective results seem to agree that the first tone in a melody tends on the average to seem raised in pitch when the melody is repeated, and the last tone seems to be lowered in pitch. This effect may be independent of any alteration of tones as in the Seashore test.

3. When one tone is raised or lowered, all the other tones in the melody tend to change along with it in the same direction. The average change in the other tones is about equivalent to one fifth of a semitone, although in some cases the change may exceed the equivalent of a semitone.

4. The dynamic effect of the altered tone is apparently greater with those tones which come before than with those which come after it. A strong retroactive effect upon those tones preceding the altered tone is established.

5. The law of proximity does not hold, except when the altered tone is raised. The maximum effect may be felt at the third tone distant from the altered tone.

6. There is some evidence that the shape of the whole melody is a potent factor in determining the phenomenal changes, both in the altered tone and in the others.

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FACILITATION OF RESPONSE TO SUGGESTION BY RESPONSE TO PREVIOUS SUGGESTION OF A DIFFERENT TYPE [1]

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It is perhaps a truism that an individual will respond to suggestions more quickly in the hypnotic trance [2] than in the normal waking state. Considerable clinical evidence has been offered in support of this postulate in the past, but only recently have experimental data been introduced in substantiation of it. The works of Hull and Huse (4) and of Williams (11), for example, indicate that while the subject is in the trance state his response to suggestions regarding swaying forward from an erect standing position takes place more rapidly than it does in the waking state.[3] However, it has also been demonstrated experimentally that a similar facilitation of response may be accomplished after the trance has been terminated, or even in the complete absence of the trance. Krueger found that response to suggestion of sleep facilitated the response to quickly succeeding suggestions

[1] This study was made possible by a grant from the Social Science Research Council to Professor Clark L. Hull. The writer is greatly indebted to Professor Hull, who suggested the problem, outlined the procedure, loaned much of the apparatus and contributed valuable advice during the course of the experiment herein described. However, Professor Hull is in no way responsible for the shortcomings of this experiment, for the treatment of data, or for the conclusions drawn from them. The writer is indebted to Mr. Don C. Koser, who acted as apparatus assistant during the experiment.

[2] The terms 'hypnotic trance' and 'trance state' are used arbitrarily in this paper to refer to the subject's condition from the instant of eye closure in response to suggestions of relaxation and sleep until the instant of eye opening in response to the signal for awakening. The distinction between this state and that of true sleep has been discussed by Bass (1).

[3] Whether this is the 'normal waking state' is of course debatable. This postural sway would probably fall within the classification of phenomena described by Professor W. R. Wells as 'waking hypnosis' (9, 10). Professor Wells (9) has demonstrated that most of the so-called criteria of hypnosis may be produced without any suggestion of sleep.

of sleep, even when the first trance was terminated (**5**). Similarly, the Hull and Huse (**4**) study indicates that response to suggestions regarding postural sway facilitates response to quickly succeeding suggestions of the same nature, both in the trance state and in the non-trance state. If one is to base any conjecture regarding the nature of hypnosis on the postulates derived from the above mentioned studies, it would also seem logical to inquire whether the converse of the first of these phenomena occurs, *i.e.*, whether response to suggestions of change of posture given in the waking state will in turn facilitate eye closure. In practice, many hypnotists, especially those who conduct vaudeville exhibitions, have assumed that the responses given in the waking state tend to shorten the time of eye closure incidental to entering the trance state. Wells (**9**) has reported that in classroom demonstrations of hypnosis he does not ordinarily suggest sleep until suggestions of contractures in the waking state have been effective. In the Hull and Huse (**4**) experiment, response to suggestions regarding postural sway seemed to facilitate succeeding eye closures, but unfortunately the time necessary for eye closure was not recorded accurately enough to justify any general conclusions.

The principal purposes of the experiment to be described here were to provide a further check on the validity of the former of these postulates by means of a different experimental set-up, and to secure data from which a postulate could be derived in answer to the latter question proposed above.

Subjects and Procedure

The S's who took part in this experiment were 9 students at the University of Nebraska, 4 men and 5 women, ranging from 19 to 24 years in age and from the sophomore year to the Ph.D. candidacy in academic rank. They were paid a small amount for their services, though they were not informed that they would be paid until after they had proved themselves hypnotizable. Before beginning the experimental procedure, 7 of the S's showed symptoms of auditory and visual hallucinations in the trance state, with amnesia for the events

occurring in it. Subject D exhibited partial amnesia for the trance state, but no visual or auditory hallucinations. Quite by accident, however, she reported kinaesthetic hallucinations. D also showed certain peculiarities, probably of an hysterical nature, in response to suggestions regarding arm movements. After the experiment began, she volunteered the information that her right arm had been broken twice and her left arm once, when she was a child. She reported some discomfort from her arm in the position in which it rested on the support during the arm suggestions, especially after the second arm movement had taken place. Subject J showed contractures and paralyses of considerable duration, but no evidences of the trance state beyond lid closure, including catalepsy of the lid muscles, and extreme relaxation of bodily musculature during the suggestions of sleep. Previous to the first experimental session J had shown symptoms of sleeping hypnosis. Later on, however, she failed to exhibit any symptoms beyond those mentioned, probably because of the development of a phobia which she was unable to explain until some time after the experimental procedure was completed. As evidence of the trance state was doubtful in her case, the data from her performances are treated separately from those of the other eight subjects. The S's were not informed of the purpose of the experiment and so far as it was possible to ascertain, they did not become aware of it.

The experiments were carried on at the Psychological Laboratory of the University of Nebraska in a semi-soundproof room approximately 9 feet square and 10 feet high. The walls of the room are solid, except for a door which was left open during the course of the experiment into an adjoining room having a large outside window. The effect of this was to provide a sufficient degree of daylight, at the same time avoiding undue noise. In most of the experimental periods a third person was present in addition to the E and the S. Usually this third person acted unobtrusively as apparatus assistant, though occasionally he merely stood at the door as witness of the procedure.

The general plan of the experiment was to compare pos-

tural changes in response to suggestion received in the waking state with the same type of movement in the trance state, and to compare eye closure before responding to the postural suggestions with eye closure after responding to the postural suggestions. The postural response employed was the moving forward of the horizontally outstretched arm from a position at an angle of approximately 45 degrees from the median (sagittal) plane of the body. The right arm was used in all cases, except that in the case of Subject D the left arm was used on the last six days. While receiving the suggestions regarding arm movement, the subject sat erect in a straight chair. The arm support,[4] or cradle, on which the S's rested their arms in order to diminish the element of fatigue during the experiment, consists of a light board, $\frac{3}{8}$ inches thick, of abbreviated sector shape with a light sheet metal wall $3\frac{1}{2}$ inches high at the arc end. The curve of the arc prevents a distortion of the record which would ensue if the end of the cradle were square. The radius of this arc is such that a movement of one inch at the outer end of the cradle is equivalent to about 1.9 degrees at the shoulder. The cradle is 27 inches long and $5\frac{1}{2}$ inches wide at the narrow end, the arc opposite measuring approximately 14 inches. Two straps hold the S's arm securely in place on the board, the small end of which is placed about $1\frac{1}{2}$ inches from the S's body at the armpit. The device is held in suspension by chains at each corner which meet in a ring 32 inches above the surface of the board. To the ring is attached a single chain which runs through a pulley on the ceiling. By adjusting the chain the cradle may be raised or lowered to a convenient height, the free end of the chain being hooked into a link slightly above the ring. At the end of the arc more distant from the recording device an eyelet at half the height of the metal arc holds a thread which runs to the recording device, the latter being in such a position that the thread is tangent to the arc of the cradle. To insure accuracy of measurement, the arm must be at an angle of 90 degrees to the thread, both being in the horizontal plane. The thread is held in the eyelet by the

[4] This device was invented by Clark L. Hull.

insertion of a small peg, so that the length of the thread may be properly adjusted.

The recording device, which provides a kymographic record of the S's arm movement, is the one previously used by Hull and Huse (**4**) and by Williams (**11**) to record postural sway. It has been described in detail elsewhere (**3, 4, 11**). As the thread is pulled by the forward movement of the cradle, the device records a vertical tracing, one third the amplitude of the arm movement, on the smoked paper. In this experiment the time was recorded at one second intervals on the smoked drum by an electric marker, the timing device being an ordinary electric clock wired to furnish one contact for the marker at each revolution of one of the shafts. The beginning of the suggestions was recorded by means of another marker, just below the tracing rod and controlled by a telegraph key located next to the starting device of the phonograph used to repeat the suggestions. The finish of the arm movement of approximately 8 inches was recorded automatically by the same marker.[5]

In order to insure uniformity of experimental conditions, all suggestions used to induce responses which were timed, were given by means of records played on a Columbia Grafonola, Type 2. Suggestions regarding sleep were given by means of Part I of Estabrooks' Hypnotic Technique Record (**2**), played at a speed such that the time from the beginning of the first word until the end of the last word was slightly more than 4 minutes. When this record preceded the arm movement suggestions, it was stopped at a point before the command to awaken, thus leaving the S in the trance state. The suggestions regarding arm movement were given by a record specially made for this experiment, the words of which began as follows:

[5] This automatic recording was accomplished by substituting an extremely fine copper wire for the last few inches of the thread, connecting with the large aluminum pulley, so that the wire made contact with the thread guide, which was insulated from the frame of the recording device, when 8 inches of thread had been pulled out by the arm movement. One wire to the marker was run from the thread guide; another, from a point on the frame near the large aluminum pulley, through the battery, to the marker.

"Your arm is resting on the support, so as to free it from fatigue. Listen carefully to what I say and pay attention to the suggestions as they are given you. Imagine how it would feel if your arm were to move forward in an arc, so as to extend straight before you. Forward, forward, forward, forward, forward. Just as if your arm were to move forward, forward, forward, forward, forward. Moving forward, forward, forward, forward, forward. All the time your arm is moving, forward, forward, forward, forward. Imagine that your arm is moving forward, forward, forward, forward, forward. . . ."

The key was not pressed by the experimenter to record the beginning of the arm movement suggestion until the word 'Imagine' at the beginning of the third sentence. The record was played only until the S's arm had moved 8 inches. Both records were repeated in their appropriate parts when the desired response did not take place within the time limits of the records.

The time of eye closure was recorded in seconds by the experimenter, with the aid of a 'split-second' stop-watch. This watch has two hands, one of which may be stopped without interfering with the other, so that it is not difficult to obtain the time for final lid closure with it, regardless of the number of preliminary flickerings of the lids. Timing began at the first word of the Estabrooks' record.

Each S served for 8 experimental periods, no two of which were on the same day. Unfortunately, the time between these performances was not always the same, though this does not seem to have influenced the results greatly. At four of the periods the sleeping suggestions were given first, followed by the arm movement suggestions; these periods will be designated hereafter as 'A' days. On the 'B' days the order of procedure was reversed, the sleeping suggestions coming after the arm movements had been completed. The A and B days were alternated for each S during the course of the experiment, except that the schedule of Subject B was in the order BAABABBA. Two men and two women began the experiment on A days, while the others, including the woman whose results were omitted from the main body of the data, began on B days. The dates of the periods of experiment and the order of procedure for each S are shown in Table I.

TABLE I

DATES OF EXPERIMENTAL PERIODS AND ORDER OF PROCEDURE FOR ALL NINE SUBJECTS

Order	A		B		A		B		A		B		A		B		A	
S.	Date	Hour	Date	Hour	Date	Hour	Date	Hour	Date	Hour	Date	Hour	Date	Hour	Date	Hour	Date	Hour
A	4/19	2 p	4/20	1 p	4/21	1 p	4/22	1 p	4/25	1 p	4/26	1 p	4/27	1 p	4/28	1 p		
B*			5/13	1 p	5/14	12 m	5/17	5 p	5/16	5 p	5/19	5 p	5/18	1 p	5/20	1 p	5/21	12 m
C			4/ 5	3 p	4/19	3 p	4/21	3 p	4/26	3 p	4/27	3 p	4/28	3 p	4/29	10 a	4/30	11 a
D	6/ 9	10 a	6/10	10 a	6/11	10 a	6/12	10 a	6/13	10 a	6/14	10 a	6/15	10 a	6/16	10 a		
E	5/12	3 p	5/13	2 p	5/14	10 a	5/16	4 p	5/17	2 p	5/18	4 p	5/19	2 p	5/20	4 p		
F	5/ 5	2 p	5/ 6	2 p	5/ 9	2 p	5/10	2 p	5/11	9 a	5/12	2 p	5/13	9 a	5/14	11 a		
G			4/28	4 p	4/30	10 a	5/ 2	1 p	5/ 3	1 p	5/ 4	1 p	5/ 5	1 p	5/ 6	1 p	5/ 9	1 p
H			5/10	3 p	5/11	4 p	5/12	2 p	5/14	10 a	5/16	4 p	5/17	2 p	5/18	4 p	5/19	2 p
J†			4/ 5	2 p	4/19	2 p	4/21	2 p	4/22	4 p	4/26	2 p	4/28	2 p	4/29	4 p	5/ 3	2 p

* The order for Subject B was BAABABBA.
† Subject J did not go into the deep trance state.

The experimental procedure for the two types of day is described in detail as follows:

A days: S was seated in a morris chair, facing the phonograph cabinet, and told to make himself comfortable. E said, "Listen to the record, but do not close your eyes until you feel yourself going to sleep." The Estabrooks record was then played, the stop-watch being started at the beginning of the first word. The time of the final lid closure was recorded. The record was stopped just before the suggestion to awaken was reached, leaving S in a trance. E then said, quietly, "In a moment you may open your eyes and come over to the straight chair, but you will still be fast asleep. You will not wake up until I have said the letters up to 'g.' Now you may open your eyes. Come over to the other chair." As S came to the straight chair, E repeated suggestions regarding sleep, to make sure that S did not waken. S was blindfolded and his arm strapped to the cradle at the proper angle from his body. At approximately three minutes from the cessation of the Estabrooks record, the arm movement suggestion record and the kymograph were started. E pressed the telegraph key at the beginning of the third sentence, as has been described previously. The phonograph and kymograph were stopped when 8 inches of arm movement had been recorded automatically. S's arm was returned to the starting position and S was told to relax his arm and rest it for a few moments. Approximately two minutes after the cessation of the first arm movement, the phonograph and kymograph were again started, the procedure being the same as before. After the second arm movement the cradle was returned to the starting position and S was directed to rest his arm, as before. After another two minutes, the arm movement procedure was repeated a third time and the cradle once more returned to the starting position. S's arm was removed from the cradle. The blindfold was then taken off and S was awakened slowly as E said the letters of the alphabet up to 'g,' the usual signal for awakening S in this experiment.

B days: S was seated in the straight chair. E engaged S in

casual conversation for approximately 3 minutes, after which S was blindfolded. The arm movement procedure was then repeated three times in the same manner as on the A day. After S's arm had been removed from the cradle, E removed the blindfold and remarked quietly, "Now you may sit in the morris chair." After S was seated in the morris chair, E gave him the same directions as were used on the A day in preparation for the playing of the Estabrooks record. Approximately 3 minutes from the finish of the third arm movement, the Estabrooks record was started and the time of eye closure recorded, as on the A day. The whole of the Estabrooks record was played, S being awakened by it. S was then excused, usually after a minute or two of casual conversation. Comments about the experiment were avoided by E, so far as possible.

Results and Discussion

The data for the days on which the suggestions regarding sleep preceded those for arm movement appear in Table II. Hereafter these days will be referred to as 'A' days, and those on which the arm movement suggestions came first as 'B' days. The data for the latter appear in Table III. Table IV contains the data from both types of day for Subject J, who did not go into deep sleeping hypnosis. While the writer has used averages extensively throughout this study to designate trends in the data, these measures of central tendency must not be taken too seriously, on account of the meagerness of the data. Probable errors have been avoided in the tables, but in a few instances where these seem to be of value they are given in the discussion of the results.

For the sake of convenience, the discussion of these data has been centered around the following specific problems:

1. Does response to suggestions of sleep facilitate later postural response? Table II shows the average time for the eight-inch movement in the trance state to be 70.5 seconds, which is 13.3 seconds less than the corresponding figure for the waking state, 83.8 seconds (Table III). That this difference, though small, is nevertheless real, is indicated by

TABLE II

A Days

Time in Seconds for Eye Closures and Arm Movements When Trance Suggestions Preceded Arm Movement Suggestions

Day		First				Second				Third				Fourth				S's Averages	
S	Sex	Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm
			1	2	3		1	2	3		1	2	3		1	2	3		
A	f	98	124	55	52	82	61	55	50	44	68	56	35	60	36	36	29	71.0	54.8
B	f	53	35	24	24	35	34	26	24	34	17	16	19	18	20	25	18	35.0	23.5
C	f	98	37	36	36	76	17	12	13	57	18	14	14	63	22	18	17	73.5	21.2
D	f	147	364	360	334	115	353	301	214	90	624	256	270	62	282	353	334	145.5	337.1
E	m	25	35	22	26	53	36	22	20	58	28	20	25	57	17	18	13	48.3	23.5
F	m	62	46	64	38	68	72	80	120	93	51	38	26	64	38	42	44	71.8	55.0
G	m	175	78	77	70	67	18	19	14	39	32	32	29	28	14	14	8	74.8	33.8
H	m	30	14	12	12	18	13	7	5	15	19	17	13	15	18	12	5	19.5	12.3
Mean.....		86.0	91.6	81.2	74.0	64.2	75.5	65.2	57.5	53.7	107.1	56.1	53.9	45.9	55.9	64.7	58.5	63.0	70.5
Median.....		80.0	41.5	45.5	37.0	67.5	35.0	24.0	22.0	50.5	30.0	26.0	25.5	58.5	21.0	21.5	17.5	60.0	28.8
Daily mean arm movement.....			82.3					66.1				72.4				59.7			
Daily median arm movement.....			37.5					25.0				27.0				19.0			

TABLE III

B DAYS

Time in Seconds for Eye Closures and Arm Movements When Arm Movement Suggestions Preceded Trance Suggestions

Day		First				Second				Third				Fourth				S's Averages	
S	Sex	Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm Movement			Eyes	Arm
			1	2	3		1	2	3		1	2	3		1	2	3		
A	f	38	102	115	113	35	100	70	65	49	53	41	46	32	73	76	64	38.5	77.3
B	f	40	49	44	41	28	34	23	20	26	58	59	43	21	24	22	20	28.8	36.4
C	f	95	44	43	40	52	20	29	18	49	18	18	16	95*	51*	32*	26*	72.8	29.6
D	f	95	294	420†	540†	90	612	376	560	30	403	247	230	58	58	97	125	68.3	330.1
E	m	19	20	22	26	53	38	31	25	45	17	18	18	53	14	20	17	42.5	22.2
F	m	65	179	148	120	52	128	37	43	52	65	78	32	51	66	44	46	53.3	82.2
G	m	21	119	217	192	37	58	56	37	28	56	43	32	15	20	16	18	25.3	72.0
H	m	51	40	26	19	25	25	18	19	25	12	10	4	13	11	12	8	28.5	17.0
Mean.....		53.0	105.9	129.4	136.4	46.5	126.9	80.0	98.4	38.0	85.2	64.2	52.6	42.2	39.6	39.9	40.5	46.0	83.8
Median.....		45.5	75.5	79.5	77.0	44.5	48.0	34.0	31.0	37.5	54.5	42.0	32.0	41.5	37.5	27.0	23.0	40.0	41.8
Daily mean arm movement.....				123.9				101.8				67.4				40.0			
Daily median arm movement.....				75.5				37.0				42.0				25.0			

* Subject C was nervous on this day as a result of an emotional upset the night before.
† Approximate time.

TABLE IV

DATA FROM PERFORMANCE OF NON-TRANCE SUBJECT TIME IN SECONDS FOR EYE CLOSURES AND ARM MOVEMENTS

Day		First				Second				Third				Fourth			
		Eye	Arm			Eye	Arm			Eye	Arm			Eye	Arm		
			1	2	3		1	2	3		1	2	3		1	2	3
A		1800*	35	46	29	2640	139	52	78	1555	43	28	32	1644	29	25	32
A	Daily mean arm mvt.			36.6				89.7				34.3				28.7	
B		345	77	79	52	282	92	70	120	1499	124	54	40	746	54	23	38
B	Daily mean arm mvt.			69.3				94.0				72.7				38.3	

	Mean Eye Closure	Mean Arm Movement
A days	1910.0	47.3
B days	718.0	68.6

* Approximate time.

the calculation of the probable error of the difference,[6] which is ± 4.2 seconds, slightly *less* than one-third the obtained difference between the means. An inspection of the data suggests that a comparison of the medians might present a truer picture of the situation than does the comparison of the means. The difference between the median for the trance state, 28.8 seconds, and that for the waking state, 41.8, is 13 seconds, almost half of the median time for the former. The performance of the girl who did not go into the deep trance state shows the same tendency, her average time for arm movements on the A days being 47 seconds, and for the B days 69 seconds. If one were to generalize from this one case throughout the study, it would seem that the same general results occur without respect to the depth of hypnosis. It should be noted however, that not all the individual S's show the same tendency as that which the means indicate for the whole group. This is shown graphically in Fig. 1, where

[6] By the formula for the probable error of difference between means where the sets of data are correlated:

$$\text{P.E.}_{M_1} - \text{P.E.}_{M_2} = \sqrt{(\text{P.E.}_{M_1})^2 + (\text{P.E.}_{M_2})^2 - 2r_{12}\text{P.E.}_{M_1}\text{P.E.}}$$

it will be noted that two of the eight subjects, D and E, had slightly higher average times for arm movements on the A days. It should be borne in mind that these S's began the experiment on A days, without benefit of previous practice.

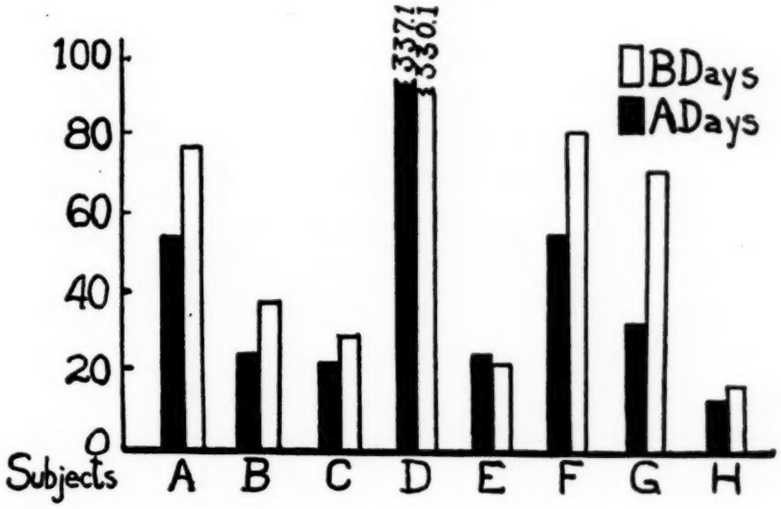

FIG. 1. Average time for arm movements in waking state (B days), compared with average time for arm movements in trance state (A days) for each subject.

Hull and Huse (4) have summarized their data to show that waking suggestion requires about two and one-half times as long as trance suggestion to bring about a given amount of postural sway. The data in Table II and Table III show much less difference in average times for arm movement than Hull and Huse (4) found for postural sway. Williams (11) discovered a smaller difference than that reported by Hull and Huse, the average time for his S's in the waking state being only one and one-half times that for the trance state. In the present study the composite medians for arm movement (Tables II and III) show substantially the same results as those obtained by Williams for postural sway, but the difference between the composite means is not so great as that found by Williams. This disparity is probably to be explained in terms of the variance in techniques employed. In the Hull and Huse and Williams studies, the suggestions were given verbally by the experimenters, while in the present experiment the suggestions were given by means of phonograph records. Although the writer has not made a statistical study of the

matter, his observations have indicated that S's respond much more quickly to his verbal suggestions than to the records. In numerous instances, S's remarked that they did not like the records as well as the voice of the experimenter, and that the records were 'impersonal.' The effect of the use of records,—which are not so conducive to 'rapport' as the voice of the experimenter and which do not adapt themselves to the particular situation,—is therefore to increase the time of response and probably to obscure differences between the average times of response in the waking and the trance states, respectively. On strictly scientific grounds, however, the writer prefers the use of records to personal verbal suggestion. Records, if used on the same machine with a constant adjustment, provide identical words and intonation for all experimental periods, which no human being is likely to do. Furthermore, records are not subject to unconscious influences arising from expectation, a factor almost impossible to control in human suggesters. On purely personal grounds the writer regards the use of records as a great convenience in saving wear and tear on his vocal organs and nervous system, monotonous verbal intonations being peculiarly tiring when continued for long periods of time with recalcitrant S's.

Another factor in reducing the magnitude of the difference between the composite average times of arm movement on the A days and the B days, respectively, seems also to be inherent in the technique employed in this experiment. With each S serving on eight experimental days, it is probable that the learning which takes place in the trance state carries over into succeeding waking states, and vice versa, thus minimizing the difference between the performances on the two types of day.[7] Regardless of what the cause may be, Fig. 3 shows that the average time for arm movements on the A days tends to approach that for the B days as the number of sessions is increased, and Fig. 4 shows the same to be true for the average time of eye closure. This constant error could probably be

[7] The writer is not familiar with any literature bearing on the problem of the transfer of this type of practice effect received in the trance state to the waking state, but one might expect it to exist. Scott (**8**) has found that certain conditioned responses set up in the trance state carry over into the waking state.

overcome by using a large number of S's, each serving for only one A day and one B day. The reader who has attempted to carry on research in the field of hypnotism will easily understand the difficulties involved in such a procedure. The writer proposes to obtain data by this procedure in the future, but of course it will take several years to accumulate enough to be of great value. If only the first A day and the first B day for each S in the present study are taken for comparison, the average time for arm movement on the A days is 82 seconds and on the B days, 124 seconds, the difference being proportionately larger than that which appears in the data for all eight experimental days. For the woman who did not go into the deep trance state the average time of arm movements on the A days was 47 seconds, as compared with 69 seconds on the B days. Her average time for arm movement on the first A day (which was her second experimental period), was 37 seconds, as compared with 69 seconds on the first B day.

In summary, the results of this study seem to support the conclusion of previous experimenters that S's respond to suggestions of change of posture more quickly in the trance state than they do in the waking state.

2. Does response to suggestions of arm movement facilitate succeeding responses to suggestions of sleep? Tables II and III show the composite average time for eye closure on B days to be 46 seconds, as compared with an average of 63 seconds on the A days. The difference is obviously 17 seconds, which is 4.7 times the P.E. of the difference ($\pm$ 3.6), when the two sets of data are correlated. The medians, 40 seconds on the B days and 60 seconds on the A days, show an even greater difference. The same tendency was present in the case of the subject who did not go into the deep trance state. Her average time for eye closure was 718 seconds on the B days and 1910 seconds on the A days. The average for J on the A days is not accurate, however, as on the first A day (which was her second experimental day), the time of eye closure was estimated. Due to the specially darkened condition of the room and her long series of eyelid flickerings, it was impossible to obtain an accurate measurement of the time. It is certain,

however, that the time for 'permanent' lid closure was not less than 1800 seconds.

Again we find that not all of the individual S's exhibit the tendency indicated by the composite averages. In the graphic treatment of the averages for the individual S's, (Fig. 2), it is evident that the performance of Subject H

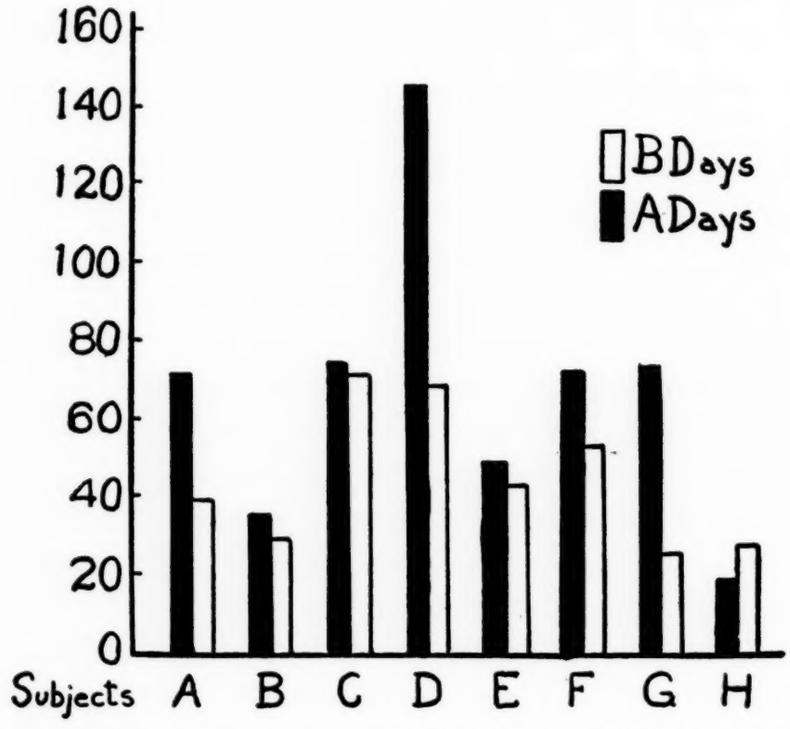

FIG. 2. Average time for eye closure after arm movements (B days), com pared with average time for eye closure before arm movements (A days) for each subject.

differed from that of the other S's, his average for the B days, 28.5 seconds, being larger than the average of 19.5 for the A days. A possible explanation of his deviation from the general tendency is that in certain cases he proved to be 'negatively suggestible'; perhaps this tendency was strengthened by the arm movement performance. The first time H listened to the suggestions regarding arm movement, his arm moved *backward* rapidly. It was only after the record was stopped, and H told to move his arm *forward* voluntarily, 'in order to get the feeling of moving your arm forward,' that he responded to the arm movement suggestion in the regular manner. After this one incident, however, his arm did not move backward again in response to the suggestions.

Previous comments regarding the possibility of constant error due to the carry-over of practice effect from one state to the other also apply to the data on eye closure (Fig. 4). If the data from only the first A day and the first B day, respectively, for each S, are taken, the average time for eye closure preceding response to the arm movement suggestions is 86 seconds, while following the response to arm movement suggestions, it is 53 seconds. In the case of the non-trance subject, the time of eye closure on the first A day was estimated, as has been explained, at 1800 seconds; on her first experimental day, which was a B day, her eyes closed at 345 seconds from the starting of the record. On the whole, therefore, the data from this experiment seem to indicate that when suggestions regarding sleep and relaxation are given shortly after response to suggestions of change of posture, the length of time required for eye closure is noticeably shortened. Since the time for eye closure is lessened, we may assume that the entrance of the subject into the hypnotic trance has been facilitated. It must be borne in mind, of course, that this generalization is based on experience with S's specially selected because of their hypnotizability.

3. Is there a practice effect? The writer has mentioned that the technique used in this experiment probably introduced a practice effect which minimized certain differences in the final results. It is of course impossible in this experiment to distinguish the effect of learning in the trance state from that of learning in the waking state. However, two types of habituation may be observed, viz., the reduction of the times necessary for arm movement and for eye closure, respectively, on successive days. The data from previous experiments is again available for comparison with the present data. Hull and Huse (4) have summarized their data for the average time of postural sway on the first, second, third and fourth experimental days, respectively, in a composite graph showing marked habituation effects. Williams' data (11) from a larger number of S's also show the same tendency, somewhat modified. In both the Hull and Huse and Williams studies the figures for postural sway in the trance and in the waking

states are combined, as the postural suggestions were given in both states on each day. It must be remembered that in the present experiment the arm movements occurred alternately in the waking and in the trance state over a period of eight days. Two methods of comparing the present data with

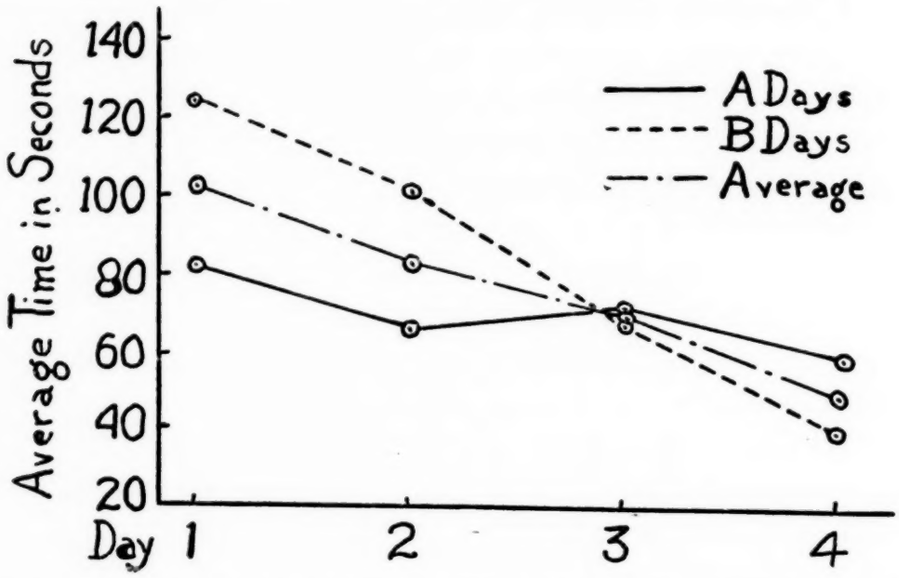

FIG. 3. Habituation effect as shown by average time of arm movements in waking (B days) and in trance states (A days). 8 subjects.

those of previous experimenters suggest themselves, either of which is likely to be misleading. The first of these is to average the curve for the A days with that for the B days, as has been done in Fig. 3. This has the effect of combining an A day with a B day for each subject, which duplicates to a

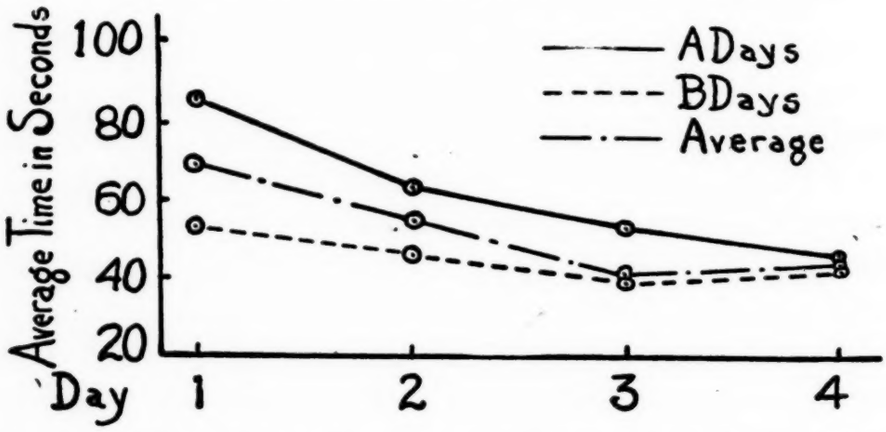

FIG. 4. Habituation effect as shown by average time of eye closure before arm movements (A days) and after arm movements (B days). 8 subjects.

certain extent the manner in which the Hull and Huse and Williams data are presented. The average curve in Fig. 3, however, represents a total of 24 trials for each S, three a day on eight days, as compared with 16 trials over a period of four days in the other studies. Furthermore, the curve in Fig. 3 is for arm movements, while the Hull and Huse and Williams studies dealt with sway of the whole body forward from an erect position. Yet despite these differences, the resulting data are not at all inconsistent. The average for the fourth A and B days respectively, as shown in Fig. 3, is 49 percent of that for the average on the first A and B days, the corresponding figure in the Hull and Huse study being 54 percent and in the Williams' study 69 percent. Another way of treating the habituation effect in the present study is to average the times for arm movements for all S's on their first experimental days, their second experimental days and so on, up to and including the eighth day, regardless of whether they began on A or on B days. In effect this amounts to pairing one S on an A day with another S on a corresponding B day. Were there not such great individual differences among the subjects, this would be justifiable, but as it is, this procedure does not show the true progression of the habituation effect. (See Fig. 5.) However, it does suggest the general shape of

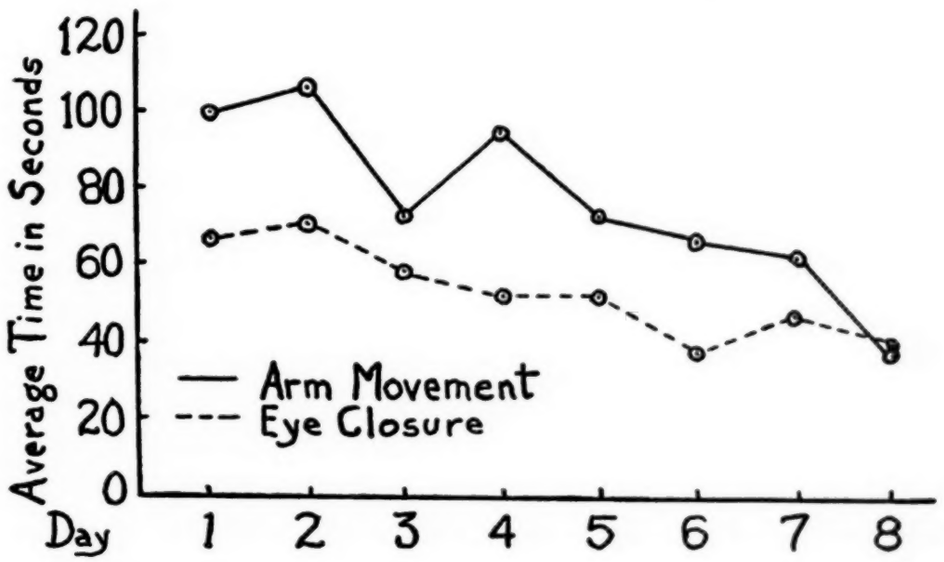

FIG. 5. Habituation in arm movement and eye closure, during eight experimental periods, as indicated by averages for the respective periods. 8 subjects.

the learning curve, the average for the arm movement on the eighth day being 37 percent of that on the first day. The average times of arm movement for the eight days, as shown in Fig. 5, were 99.7, 106.4, 72.8, 95.0, 73.2, 66.5, 62.3 and 37.0, respectively. A curve drawn from the medians, instead of from the means, would be much smoother.

The habituation effect in the case of eye closure has been studied by Krueger (**6**), who found that on the tenth day, the average length of time necessary to produce eye closure incident to entering the trance was about 26 percent of the average time necessary on the first day. The curve in Fig. 5 for the eight days of the present study (computed in the same manner as the corresponding curve for arm movements), shows a tendency similar to that found by Krueger, but less pronounced. The average times of eye closure for the eight days were 67.4, 71.6, 58.4, 52.4, 52.6, 39.1, 48.4 and 39.7, respectively. In Krueger's study the average time of eye closure on the eighth day was shown to be approximately 30 percent of that on the first day, while in the present study the average for the eighth day is 59 percent of that for the first day. The conditions were of course not the same in the two experiments. On half the days in the present study the eye closures were facilitated by the preceding arm responses, which would lead one to expect that our curve would drop more rapidly than that of Krueger, instead of less rapidly. One must remember, however, that in the present case, the suggestions for eye closure were given by means of a phonograph record, the general effect of this being to increase the time of eye closure. Another characteristic of the present study is that several S's had been hypnotized a number of times previous to the beginning of the experiment. Even then, the general tendency of habituation in eye closure is similar to that found by Krueger.

The curves for average time of eye closure in Fig. 4 should be mentioned again here. As has been pointed out previously, the average times for the A days and for the B days respectively, tend to approach each other as the number of experimental days is increased. A comparison of the curves

for the A days and for the B days, respectively, in Fig. 4 with the corresponding curves in Fig. 3 leads one to observe that in both arm movement and eye closure, habituation proceeds more rapidly in the periods which are not preceded by response to other suggestions. If one were to generalize from these meager data, it would seem that when a response has been facilitated by a response to a previous suggestion (or in other words, if a response takes place in a state of either waking or sleeping hypnosis), it approaches the limit in point of speed which would otherwise be attained only by practice. The ramifications of such a postulate (as well as of others which might be mentioned here), would be far reaching indeed, but since the evidence on which it could be based is so limited, the writer refrains from discussing them. He feels justified in mentioning this aspect of the experiment, however, in the hope that it may prove of value to other investigators, just as mention of minor points in previous studies has been helpful to him in the conduct and interpretation of the present experiment.

4. *Do succeeding arm movements facilitate each other?* The Hull and Huse (**4**) and Williams (**11**) studies contain evidence of a cumulative effect when the suggestions for a second postural sway are given soon after the first sway has taken place. They report that on the average the second postural sway response required about 75 percent of the time necessary for the first response, both in the trance and in the waking states. Williams reports substantially the same results, except that the second response took place, on the average, in about 84 percent of the time necessary for the first response. The data from the present experiment bearing on this problem have been summarized graphically in Fig. 6. The average times for the arm movements were 89.4, 78.4 and 82.0, respectively, for the B days and 82.5, 66.8 and 61.0 for the A days. The averages for A and B days combined, for the first and second arm movements, indicate the same trend as that found by Williams for postural sway, viz., the second arm movement required, on the average, 84 percent of the time necessary for the first one. In the case of the present data,

however, the facilitation seems to have been greater on the A days than on the B days, the second arm movement requiring, on the average, 81 percent of the time necessary for the first movement on the A days, and 87 percent on the B days. For the third arm movement, Fig. 6 would seem to

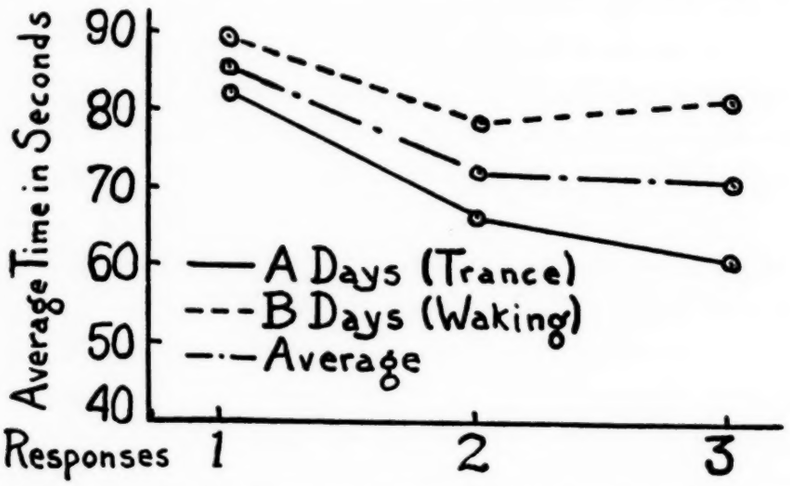

FIG. 6. Cumulative effect as shown by average time of response to first, second and third arm movements, respectively. 8 subjects on each of eight days.

indicate a divergency of tendencies on the A and B days, respectively. The average time for the third arm movement on the B days is greater than that for the second, while on the A days, the average time for the third arm movement is less. The medians, however, do not show evidence of such a divergency. Although the problem under discussion is relatively unimportant, particularly in view of the small number of cases involved, yet it seems desirable to pursue the analysis of the data a step farther in order to understand more fully what happened in the experimental sessions. Table V shows the average time which each S took for the first, second and third arm movement, respectively, both on A days and on B days. On the A days, six S's averaged less time on the second arm movement than on the first one, while Subject G had the same average for both first and second arm movements; Subject F increased his average time from 51.75 seconds to 56 seconds. On the B days, six S's showed a reduction of average time on the second day and one, Subject E, increased his average time by one half second. Subject G increased

TABLE V

AVERAGE TIMES IN SECONDS FOR FIRST, SECOND AND THIRD ARM MOVEMENTS, RESPECTIVELY, FOR EACH SUBJECT, IN TRANCE AND WAKING STATES

S	A Days (Trance)				B Days (Waking)			
	1	2	3	Mean	1	2	3	Mean
A.....	72.2	50.5	41.5	54.8	82.0	75.5	72.0	77.3
B.....	26.5	22.7	21.2	23.5	41.2	37.0	31.0	36.4
C.....	23.5	20.0	20.0	21.2	33.2	30.5	25.0	29.6
D.....	405.7	317.5	288.0	337.1	341.7	285.0	363.7	330.1
E.....	29.0	20.5	21.0	23.5	22.2	22.7	21.5	22.2
F.....	51.7	56.0	57.0	55.0	109.5	76.7	60.2	82.2
G.....	35.5	35.5	30.2	33.8	63.2	83.0	69.7	72.0
H.....	16.0	12.0	8.7	12.3	22.0	16.5	12.5	17.0
Mean.....	82.5	66.8	61.0	70.5	89.4	78.4	82.0	83.8
Median....	32.2	29.1	25.7	36.6	52.2	56.2	45.6	54.2

his average time from 63.2 to 83 seconds, although on three of the four B days his time for the second arm movement was shorter than that for the first one. The data for the third arm movement on the A days may be summarized to show that five S's reduced their average time for the third arm movement from that for the second, while one had the same average and two increased their averages from the second to the third arm movement. These two increases were practically negligible, being one-half second, and one second, respectively. However, the medians for the second and third arm movements are the same. On the B days, seven of the subjects show a lower average time for the third arm movement than for the second. Subject D shows a different tendency, her average mounting from 285 seconds for the second arm movement to 363.75 for the third one. The increase in average time for the third arm movement over that for the second for the eight subjects, as shown in Fig. 6, may therefore, be charged directly to Subject D. She was the S who complained frequently that the position of her arm on the cradle was uncomfortable, particularly toward the end of the arm movement procedure. It seems reasonable to suppose that this interfered with her response to the suggestion of movement, thus introducing a constant error in her data which was not present in the results from the other S's.

On the whole, therefore, this study seems to support the conclusions of previous experimenters regarding the cumulative effect when one response to a suggestion of postural movement shortly follows another. There is also some evidence that, in general, the cumulative effect carries over from the second to the third arm movement. It is possible that a long series of arm movements of this type would show a negatively accelerated curve, if the element of fatigue were properly controlled. This finding is only at slight variance from that of Krueger (**5**), who found the average time for the third postural sway to be .05 second longer than that for the second sway. On the fourth test, however, his subjects averaged over two seconds less than on the third day.

5. *Are the degree of susceptibility to one type of suggestion and the degree of susceptibility to succeeding suggestions of a different type related?* The writer has frequently observed what seems to be a relationship between the length of time it takes an individual to go into the hypnotic trance and the speed of response to suggestions given him while he is in the trance state. This agrees, of course, with the supposition that the more quickly a subject 'goes to sleep,' the deeper the trance will be. Although the number of S's in the present experiment is too small to yield conclusive evidence, the fact that its results support the above mentioned observation is of interest. The coefficient of correlation by the rank difference method between the average time of all eye closures and the average time of all arm movements for each S on the A days (as shown in the last two columns of Table II), is + .63. This suggests that the S's who were most readily hypnotized were most responsive to suggestions given in the trance state. Another way of approaching much the same problem is to correlate all the eye closures on the A days (32 in number), with all the average arm movements for the same days. The coefficient in this case by the rank difference method is + .67 ± .07. This would seem to indicate that the degree of susceptibility shown going into the trance was related to the degree of suggestibility in the trance.

One may also ask whether a similar relationship exists

between the average time of arm movements and of eye closures on the B days. Here the relationship is not so pronounced. The coefficient of correlation derived from the last two columns of Table III is + .33. When all 32 eye closures on the B days are correlated with the corresponding average arm movements, the coefficient is + .35 ± .11. Evidently the response to suggestions of postural change does not facilitate the trance response in such direct proportion as the latter facilitates the postural response.

Even though the coefficient of correlation between the average time of arm response for each S and the corresponding average eye closure on the B days is low, it is nevertheless positive, which might lead to the conclusion that a relationship exists between the suggestibility of persons in the normal waking state and their hypnotizability. This would be contrary to the theory advanced by W. R. Wells in a recent article (**10**). The results of an experiment in which he correlated the rankings of S's in hypnotizability with their standings in tests of suggestibility, extroversion and ascendance seemed to indicate that the more hypnotizable persons are even less suggestible in every day life than the less hypnotizable. This apparent contradiction between the findings of the present study and those of Professor Wells depends, however, on the assumption that the response to the arm movement suggestions used in the present research is a criterion of suggestibility. Wells would undoubtedly consider this phenomenon not as a criterion of 'suggestibility,' but as one of 'waking hypnosis.' According to his point of view the positive coefficient of correlation between speed of response to suggestions of postural change and speed of response to suggestions of eye closure incident to going into the trance, would therefore not indicate a relationship between ordinary suggestibility and hypnotizability, but rather, it would seem to demonstrate the likeness of 'waking hypnosis' and 'sleeping hypnosis.' The problem still remains one of the definition of suggestibility.

It is not the intention of the writer to discuss in detail the nature of suggestion and hypnosis, or of suggestibility; yet

several comments seem pertinent at this juncture. To state the matter briefly: Morgan (**7**) has defined suggestibility as "an attitude or set which makes a person amenable to a wide range of stimulus situations." He intimates that the explanation of suggestibility is to be found in terms analogous to those used in describing the facilitation of simple reflexes. According to this point of view, hypnotizability and suggestibility are closely related, if not identical. The present study indicates that, in the case of certain individuals, an attitude such as that described by Morgan may be built up or induced by succeeding responses to promptings regarding postural change. It would seem reasonable to refer to this condition as 'suggestibility,' which, as has been previously mentioned, is related to 'hypnotizability.' Professor Wells (**10**) on the other hand, has held that suggestion and hypnosis have little in common, suggestion being characterized by indirectness and evasiveness, while hypnosis involves extreme directness. The arm movement procedure employed in our experiment and the postural sway technique used by Hull and others (**4, 5, 11**) would probably be classified by Wells as waking hypnosis because of their directness. Yet if one were to overlook the manner in which the phenomenon was brought about, and were to describe it in terms of the condition itself (*i.e.*, as an attitude or set which renders the subject amenable to a wide range of stimulus situations), Morgan's term 'suggestibility' would describe it accurately. Thus, in this case, if one were not too strictly bound by the rules of logic, suggestibility and waking hypnosis would appear to be one and the same thing. The results of the present experiment would seem also to indicate (to the satisfaction of the writer at least), that the condition of suggestibility induced by a previous response is identical with that of waking hypnosis. Furthermore, since the arm response facilitates 'going to sleep' in practically the same manner as the latter facilitates the arm response, it is difficult to see how the 'waking suggestion' and 'trance' states differ, except that in the latter condition the subject has relaxed, as he does in sleep (thereby removing inhibitions), and ceased

(as he also does in sleep), to carry on certain perceptive functions. The effect of these 'sleep' tendencies is of course to increase the suggestibility of the subject, *i.e.*, to put him into an attitude which makes him amenable to a wide range of stimulus situations. Though it may not yet be evident, the point which the writer wishes to stress is that when certain minor compromises are made, the viewpoints of Morgan, Wells and Hull concerning the nature of suggestibility and hypnosis, as understood by a novice, are not so greatly at variance as Wells's article (**10**) might lead one to suppose. Whether anyone will agree with the writer, is another question.

Summary

The following statements must not be regarded as broad generalizations. They are based for the most part on the performance of only eight subjects who were selected because they proved to be hypnotizable. The fact that they could be hypnotized places them in a special class; it cannot be assumed that the same results would be found with an 'unselected' group of subjects.

1. This experiment demonstrates the feasibility of using phonograph records to induce the hypnotic trance and to convey other suggestions in the investigation of hypnosis and allied conditions, thus providing freedom from a source of error which hitherto has been difficult, if not impossible, to control.

2. In this experiment it was found that, in general, response to suggestions regarding the forward movement of the outstretched arm took place more rapidly in the trance state than in the waking state. That is, the response to suggestions regarding eye closure seemed to facilitate response to succeeding suggestions regarding arm movement. These data support the conclusions from previous experiments on postural sway.

3. The converse of the preceding tendency was also evident. On the average, the eye closure incident to going into the trance took place more rapidly after response to suggestions regarding arm movement than it did when no such response preceded the suggestions of sleep.

4. When subjects responded three times in succession to suggestions regarding arm movement, there was evidence of a cumulative effect, though the facilitation of the third movement by the second was not so marked as that of the second by the first. In general, this agrees with the findings of previous investigators regarding postural sway.

5. Habituation is evident in the responses on succeeding days, both as regards eye closure and arm movement. A curve of the results of practice would show negative acceleration.

6. The data seem to indicate a positive relationship between the length of time necessary for eye closure and the length of time involved in arm movement, when the suggestions regarding sleep were given first. A similar positive relationship seems to exist when the procedure is reversed, though the coefficient of correlation is much lower in the latter case.

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IS THE BLIND SPOT BLIND?[1]

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In the spring of 1926, while teaching at the University of Kansas, the writer became interested in reports of the perception of apparent movement in the blind spot and set up apparatus to demonstrate these phenomena. Between May 1 and May 10, 1927, he recorded observations of apparent movement made by six observers including himself. The others were Raymond Wheeler and Harry Helson, professors of psychology, Samuel King, assistant instructor of psychology, Mark Ballard, instructor of physiology, and C. C. Baldwin, graduate student of education and county superintendent. The phenomena observed included beta, gamma, and delta movement, and pure phi. Stimulation was given by two spots of light exposed in quick succession and also by the reflections onto the background from a white card held in front of these spots. Since there was a high incidence of failure in these experiments it became apparent that optimal conditions were hard to maintain. This led to consideration of the controversies over (1) what the optimal conditions for movement are and (2) the visual properties of the blind spot. A study on the first of these questions is recorded in a thesis.[2] The other led to the investigation reported here.

The question offered as the title of this article can be interpreted in two ways, both of which represent points which have been controversial. (1) Is it impossible for a stimulus which lies entirely within the blind spot to be perceived at all?

[1] From the Psychology Laboratory of the University of Kansas. Acknowledgment is due Dr. R. H. Wheeler and Dr. Harry Helson for stimulating interest in problems of the blind spot.

[2] C. R. Garvey, *An Experimental Investigation of the Relation Between Exposure-Times and the Interval Between Exposures in Apparent Visual Movement*, Lawrence, University of Kansas Library, 1927; The relation of exposure times to the interval between exposures in apparent visual movement, *Psych. Bull.*, 1931, **28**, 697–698.

(2) If not, is it perceived by virtue of stimulating sensitive elements outside the optic disk, *i.e.* is the optic papilla visually insensitive to stimulation by light of ordinary intensities? That the first question must be answered in the negative can readily be seen by reading the articles of Helson,[3] Scofield,[4] DeSilva and Weber,[5] and of the authors to which they refer. In fact, Scofield's results are conclusive by themselves. The controversy over the second question, *i.e.* the sensitivity of the optic disk, has emerged with renewed vigor with the article of Helson. He reviews the literature, and presents data of his own which lead him to believe the optic disk sensitive, either by virtue of end organs existing in the papilla or by virtue of the sensitivity of the optic nerve axons in this region. DeSilva and Weber present additional data and review the controversy, presenting rather convincing arguments to show (1) that the sensitivity of the optic disk is not substantiated and (2) that it is not necessary as an explanatory factor. The writer wishes to present data which apply to this question and which he believes to be, by virtue of the nature of the stimuli employed, more crucial than any test of this point yet reported.[6]

[3] Harry Helson, The effects of direct stimulation of the blind-spot, *Am. J. Psych.*, 1929, **41**, 345–397.

[4] C. F. Scofield, Perception in the region of the optic disk, *Am. J. Psych.*, 1930, **42**, 213–234.

[5] H. R. DeSilva and Alden Weber, The responsiveness of the blind spot, *J. Exp. Psych.*, 1932, **15**, 399–415.

[6] At this point it might be well to note that there has been considerable confusion in definitions of the term blind spot, its having been used ambiguously to designate the optic disk, the optic papilla, and the 'blind' spot in the field of vision. The author wishes to confine its use to the spot in the field of vision, which is produced by the properties of the optic disk. The latter he hopes henceforth to call only by its unique and proper name. This is in accordance with precedence set by Hunter's 1919 text (p. 250) and with the principal part of the definition in the prepublication edition of Warren's dictionary, which also gives the distinction between optic disk and optic papilla. It is hoped hereby to avoid the necessity for such cumbersome terms as the 'projection of the blind spot,' the 'blind spot of the visual field,' the 'blind spot of the retina,' the 'physiological blind spot,' the 'optic disk in the field of vision,' the 'region of the blind spot,' and the 'area corresponding to the projection of the blind spot,' as well as to allow the reader always to know whether reference is made to the retina or to the visual field. The present paper now needs a supplementary title, 'Is the optic disk blind?'

Stimuli

In order to test the theories of perception of objects in the blind spot we must use form discrimination rather than mere form perception. If the sensitive substance theory of Helson is true, then the most favorable conditions for successful form discrimination are those which involve forms which stimulate *different* areas of the optic disk. *Two forms whose areas are mutually exclusive would be ideal.* If the irradiation theory is true, then the most favorable conditions do not concern the non-identity of particular areas, but involve forms which produce differential intensities at different points on the periphery of the optic disk. Thus it would be the approach of the corners of a square toward the four points on the periphery of the optic disk which would produce the effect of four-corneredness, the gradients of the corners assisting to complete the square. The most *unfavorable* conditions under this theory would be the presentation of two forms which have no differences in such prominences as corners etc., which, if we may say so, are the same 'shape.' That is, under the irradiation theory the most hopeless comparison would be between two forms which present the same corresponding degrees of irradiation in all respective directions toward the periphery of the optic disk. If the forms had *no* irregularities of outline such as corners, so much the better (more hopeless). *Two circles would be ideal.*

Let us construct two forms which fulfill both the most favorable conditions for form discrimination under the sensitive disk theory and the most unfavorable conditions under the irradiation theory. In Fig. 1 we find a dark disk inside a light ring. The ring and disk have the same area, they have the same center hence the same position, and they have the same shape of external outline, yet they are mutually exclusive in area. In Fig. 2 the disk is light instead of dark but it is the same size and shape as in Fig. 1 and when superposed has the same position. The ring of Fig. 1 and the disk of Fig. 2, when placed in the same position (alternately), are as similar as it is possible for them to be without overlapping in area, and hence should be indistinguishable on the basis of

irradiation, except insofar as Fig. 1 is larger and approaches closer to the retinal end organs and might thus appear brighter. They are as different as is necessary according to the sensitive disk theory, since the prime requisite here (as with retinal vision) is mutual exclusiveness of areas. According to this theory therefore, they should be readily distinguished from each other, even in the blind spot, whenever their intensities

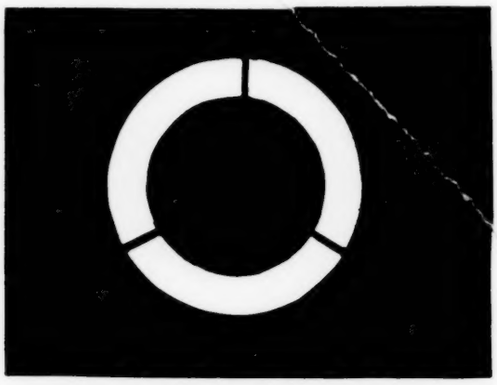

FIG. 1

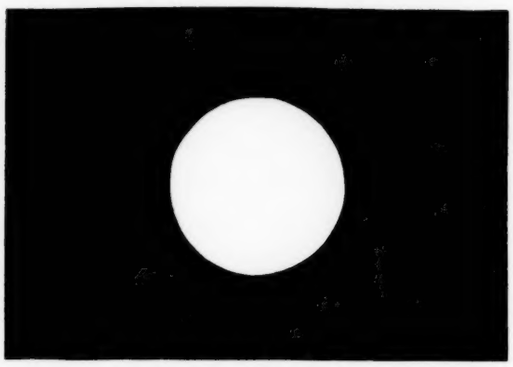

FIG. 2

are sufficient to render them perceptible. In order to test these two assumptions, *i.e.* to see which theory is true, we performed the following experiments. If the irradiation theory is true the forms may not be distinguishable in the blind spot; if the sensitive disk theory is true they *must* be distinguishable, and on the basis of form, not brightness etc.

APPARATUS

The experiments were carried out in the dark room at the University of Kansas between May 18, 1927 and July 9 inclusive. There were six subjects, five of whom completed the series. They were Raymond H. Wheeler, Harry Helson, C. C. Baldwin, S. H. Bartley, assistant instructor of psychology, Stanley B. Lindley, and George A.

Kelly, graduate students of psychology. These were all careful and conscientious observers who gave serious attention to the work, two of them (W, H) being highly practiced introspectionists and the others more naive in various degrees. Helson has already vouched for the integrity of some of them for this sort of work.

The subject sat, with his forehead resting against a curved head rest and his teeth embedded in an individual hard wax dental impression, facing at a distance of 3 meters a screen 1 meter square in the plane of which was a fixation light consisting of a dull red cross. The screen consisted of diffusing glass covered with dull black paper except for a 10 cm opening in the subject's right blind spot, for insertion of stimulus cards. Behind this opening was a 300 watt lamp housed in a light-tight water-cooled box which allowed the light to shine on the screen at the opening but prevented its escaping into the room. The center of the filament was 56 cm from the glass. In order better to disperse the light and make the illumination of the glass homogeneous, a bi-convex lens of 20 cm focal length was inserted directly in front of the source lamp. Three 1.1 ampere, 170 ohm variable resistances were wired in parallel with each other and all three in series with the source lamp. There was one such rheostat in series with a 200 watt room lamp which was enclosed in a frosted globe on the ceiling. Notes were taken with the aid of a small desk having a hood which concealed a dim light illuminating only the area of the experimenter's note paper. So far, the equipment was essentially the same as that used in the study of apparent movement previously mentioned. The additional features were black cardboards 16 × 20 cm, out of which Figs. 1 and 2 had been cut with diameters of c. 58 and 82 mm, with arrangements for quickly inserting either card in a rigid position on the front of the screen over the opening. A small projection from the head rest prevented stimulation of the left eye.

Procedure

Each subject's blind spot was mapped on a separate large black card which could be anchored in known position against the screen. The room light was dimmed and the limits of the blind spot were determined along 16 radii by the movement of a piece of white paper 2 cm square pasted near the end of a small black stick. Points of appearance and disappearance were not averaged but both were plotted. Stimuli were kept well within the inner row of plotted points. Slight shifts of the fixation light and of the stimulus card holder allowed for adjusting the center of the card to the center of the blind spot. The subjects were practiced in maintaining fixation upon the red cross while attending to and formulating judgments upon objects in and near the blind spot. The control of head and eye movements is very important and great care was taken to attain such control. Lengthy description will not improve what success was had, however. Suffice it to say that the subjects were trained; that the red cross is easier to fixate for long periods than a plain round black or white spot; that at 3 meters limited eye movements can be allowed without getting the 8 cm stimulus outside the blind spot, which is probably not less than three times this diameter; that the nature of the negative results obtained are a self-contained check upon stimulus control, since loss of fixation would have resulted in retinal stimulation and identification of the stimulus; that this last point is attested by the fact that in a few isolated instances in which two of the subjects gave descriptions which resembled the stimulus, they usually reported some disturbance of the eye muscles at the same time; and that the policy of allowing the subject rather than the experimenter to signal for the stimulus avoided giving the stimulus when the subject was not ready.

Ten trials were given to each subject, and more to those who found the first

trials subliminal. Each trial consisted of the presentation of Fig. 1 and then of Fig. 2. In the first trial the figures were rather dim and in each succeeding trial were made more and more intense by changing the rheostats between trials by amounts which had previously been found to produce apparently equal steps in illumination of the figures. There were 11 such steps, the last two being used only in case they were required to make 10 visible trials. The twelfth position had no resistance in series with the source lamp and consequently amounted to illumination of the glass screen through the cut-out portions of the figures by the full intensity of 300 watts through a dispersing lens at a distance of 56 cm.

The subject was dark adapted for 20 minutes during which the apparatus was made ready for the particular subject and the subject and experimenter conversed on various topics including the importance of holding fixation, and of reporting as completely as possible a full description of everything that was seen. The subjects did not know what stimuli were to be presented nor the nature of the experiment. One subject thought it was to be an apparent movement experiment and was disappointed not to be able to report movement. None of them knew that it was a form experiment. They were told that stimuli would be given in pairs and that they were to describe fully both members of a pair and compare them with each other in all respects. This they did, giving spontaneous information on form, size, color, brightness, patterning, concentration, irregularity, definiteness, and other qualities unclassifiable. Ignorance of the stimuli is very important in an experiment of this kind. Previous knowledge would surely have handicapped our subjects in giving original descriptions, and would probably have detracted from the wealth and variety of detail obtained from such simple stimuli.

When the subject was ready he gave a signal, at which the experimenter turned on the source light and quickly removed a black card exposing the 1st stimulus, Fig. 1. When the subject felt that he had observed the stimulus long enough to enable him to describe it he signaled again and the experimenter covered Fig. 1 with the blind card, removed Fig. 1, inserted Fig. 2 and removed the blind, exposing Fig. 2. This required between 3 and 5 seconds, during which the subject winked a couple of times, regained his fixation, and signaled for Fig. 2 as soon as he was ready. He signaled for the termination of the exposure, whereupon the source light was switched off and the subject reported. Some of the subjects were able to talk without losing their grip on the biting board and gave running accounts. Between trials the room lights were not turned on, but the subject was allowed to use the dim glow on the desk pad if he wished to write or draw his report. Some of the drawings are shown in Fig. 3. All ten trials were given in a single session, which require between 60 and 90 minutes. At the end of the series the subjects were shown the stimuli in direct vision, but were unable to recognize them. They were assured that these were the only stimuli used and that they were always given in the same order. They were then asked to name the order, whether ring first and then disk or *vice versa*. All 5 subjects were mistaken.

Observations

Some sample phrases which may be of value in portraying the stimuli descriptively are taken from the records of each subject. While reading these descriptions it should be remembered that '1st' is the ring and 2d the disk, and that the intensity increases from trial to trial.

(Bt) Didn't see a thing (1st and 2d). 1st nothing, 2d slightly lighted area irregularly circular, lasted about 2 sec. 1st (medium intensity) about 7 cm diameter, no boundary, shades off into darkness, then disappears and reappears—grayish white. A glow without definite boundary. Motley—speckled. Cottony or wooly texture. Cloudy. Not a disk,

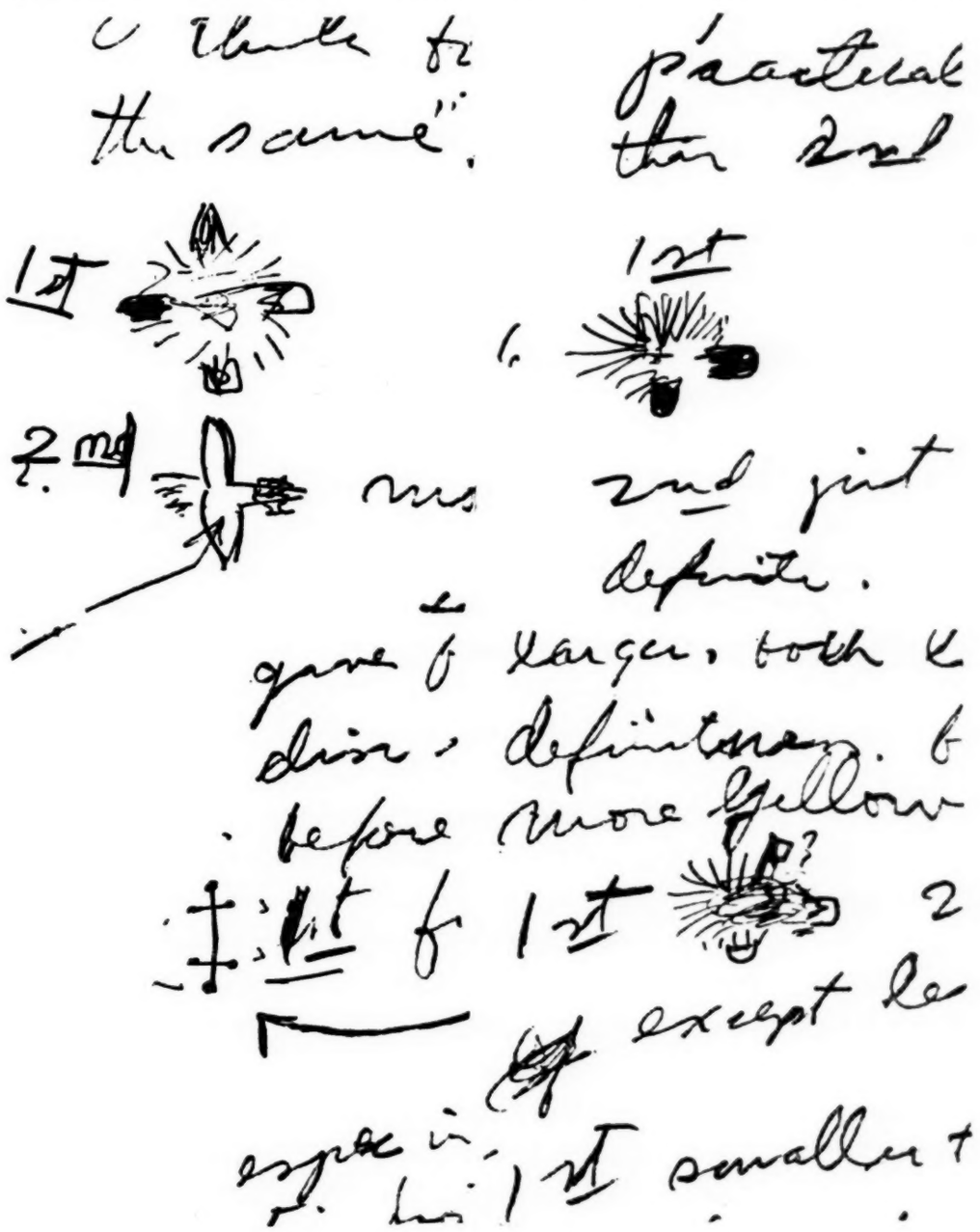

FIG. 3. Sample photographic reproductions of unretouched drawings made from observation of the stimuli in the blind spot without knowledge or previous observation in direct vision.

no very definite shape. 1st a diffuse patch of grayish white light roughly circular but no definite boundary although there seemed to be a tendency for a disk to rise out of the patch in some parts but very dimly, 2d same except no disk. Can't see much difference. Can't say about relative size because of indefiniteness.

(W) Tridimensional—light comes toward me as in a fog. Like a headlight in a fog. Rectangular—long up and down. Bursts of light in different directions without center. "I think both stimuli are the same." There are nuclei (2d), the thing is irregular—neither circular nor square.

(H) A pattern of color that was orangeish but with other colors in it, perhaps green, like a peacock's tail, mottled, speckled.

(K) 1st nothing, 2d same thing. Very thin dim light like the dim moon through cloud—very much diffused as through a very thick white cloud—a rather circular blur but no outline. 1st and 2d same. Real bright—dazzling. Hard to tell where center of illumination is. Exactly same. No boundary between light and diffusion (matter of degree). 1st too bright—hope you don't make them any brighter—dazzling—no shape, 2d same except dimmer and redder—very little difference etc.

(Bd) Glow like the pale moon—diffused light. Just a light glow—no definite shape except rather round. Just a spread of light—no definite shape, outline, or nucleus. A lighted area with rays extending in all directions. Like halo around white way light. 1st and 2d no difference. Too bright—hurts my eye and makes it water—so bright it isn't even funny—but it is a filtered light it seems. 2d gave a larger range than the 1st but seemed to be softer and not quite so intense.

(L) 1st nothing. 2d toward last thought I saw a grayish shadowy line. Felt a gray mass grow out—no size or shape, probably spherical. Sort of light, no definite shape or size, like very faint clouds. Just a 'general' light—gray. By golly you don't want to blind me. 2d exactly same as 1st—no form—too strong.

We found evidence which seemed to corroborate Helson with regard to form perception. On the other hand, while the only stimuli used were circular, we obtained reports of various forms such as a cross, plus sign, rectangle, vertical with two cross bars like one plus sign above another, crooked line like lightning, curved line like a crescent, multiple nuclei, astral shape, and speckled area. More often the reports were of indefinite, irregular, and ill-formed brightness areas (sometimes volumes). The only times eye movements were reported there was also reported the perception of some sort of a ring. One rather naive subject reported a doughnut-shaped glow and "felt a pull on the eye just like is felt in looking at the staircase illusion." The more practiced and the more stolid individuals did not report such definite rings. There are also a few reports which might be thought to substantiate Helson's statement that there is some perception of form in the blind spot if it can be seen before diffusion occurs. For instance, "Began as a ring, then shrank to a spot one-half the size and left a halo around it." Unfortunately the stimulus was not the ring but the disk. Again, when the ring was shown a disk was said to have a tendency to arise, then when the disk was shown it was said to be the same except with no disk. Again, the ring was described as a 'homogeneous disk of faint white light' then the disk stimulus as 'tendency to be a ring at first.' Another example is, "disk for an instant, then becomes diffused." But the same subject in another trial saw this in reverse order. This was from the same subject who in Helson's experiment was positive that form was given *perfectly* at the beginning of stimulation but that its rapid disappearance made it hard to detect. One subject described the 1st stimulus as circular and the 2d as rectangular for 2 or 3 trials then said the order was changed. The next trial he said, "Again you are giving the 2d figure 1st." Later he decided that there must have been only one stimulus figure all along.

The results that could be classified statistically are shown in Table 1. This table shows that with the exception of brightness, the two stimuli were so similar in the qualities named in the table as not usually to provoke judgments of

TABLE 1

COMPARISON OF 2D STIMULUS (DISK) WITH 1ST (RING)

Subject		W	Bd	Bt	L	K	Totals	
In brightness	Brighter	2	3	3	0	0	8	8
	Dimmer	7	1	4	4	8	24	24
	Same	1	1	1	0	0	3	18
	No difference	0	5	2	6	2	15	
In size	Larger	7	4	4	0	0	15	15
	Smaller	1	0	2	0	5	8	8
	Same	2	1	2	1	1	7	27
	No difference	0	5	2	9	4	20	
In concentration	More	1	1	0	0	0	2	2
	Less	6	4	6	2	0	18	18
	Same	0	0	0	0	0	0	30
	No difference	3	5	4	8	10	30	
In form definiteness	More	2	0	0	0	1	3	3
	Less	7	1	4	2	1	15	15
	Same	1	0	0	0	0	1	32
	No difference	0	9	6	8	8	31	

difference. The heading 'same' indicates that the subject declared the two stimuli to be equal in that quality. The heading 'no difference' includes general statements that the two stimuli were no different from each other (except in other respects than the one under consideration), as well as cases in which difference was not mentioned (presumably because none was noticeable). These data may be summarized as in the last column of totals. The reader will remember that a possible explanation for the difference in brightness was anticipated, on the basis of the irradiation theory. This agrees with the results of Scofield and of DeSilva and Weber and confirms their support of that theory. Another explanation is to be found in the difference in degree of light adaptation during the 1st and 2d stimulations of each trial. This is undoubtedly a contributing factor and the experiment should be repeated with the order of stimuli reversed to convince those who may attribute greater import to this factor than does the writer.[7] These skeptics should not be found

[7] An interesting improvement upon the present set-up would be to illuminate the two figures by separate sources, so that the brightnesses could be equated. This would also permit of more suddenly changing from one figure to the other. They could be alternated rapidly, thus permitting more nearly simultaneous as opposed to successive comparison.

among the supporters of the sensitive disk theory, however, because according to this theory the two stimuli were stimulating different sensory elements in different areas of the optic disk and the action of one stimulus should not adapt the receptors responsible for the perception of the other. It is only on the basis of the irradiation of light to identical retinal organs that considerable additional adaptation should be expected. At any rate, the conditions and procedure were not such as would cause the 2d stimulus to appear very noticeably dimmer than the 1st, even in direct vision. We believe the difference is due to the fact that the 2d (disk) had to penetrate to a greater distance and through a greater amount of insensitive optic nerve substance to reach the retina. This would also be consistent with the other differences shown in the table. Although there was usually no difference reported, the 2d (disk, smaller, more concentrated, simpler form) was reported more often larger than smaller, more often more diffused than more concentrated, and more often less definite than more definite. It looks larger because it is more diffuse and less concentrated. The other one is more concentrated and definite because it is nearer the sense organs and filtered through less tissue. These differences explain why each subject misidentified the two stimuli when he saw them in direct vision for the first time, after his experiment was finished.

Conclusion

In conclusion we may say (1) that while certain statistical differences were noted in the descriptions of the two stimuli used in these experiments, these differences were small and such as might be expected on the basis of irradiation. (2) Most of them are contrary to what should be expected on the basis of sensitive substance in the optic disk. (3) The figures were not recognized and distinguished on the basis of form as was expected according to the sensitive disk theory, but were practically indistinguishable as the irradiation theory would permit. (4) The irradiation theory is consistent with the facts we have found. (5) The sensitive disk theory is neither sufficient nor necessary for the explanation of blind spot

vision. For this purpose the anatomists are relieved of the duty of finding rods and cones in the optic papilla and the physiologists of demonstrating the visual sensitivity of optic nerve fibers to moderate intensities of light.

Discussion

With respect to theories of physical *vs.* physiological irradiation, we conceive of the irradiation as mainly physical. Helson has pointed out the fallacies in the physiologic theories. While Scofield speaks almost entirely in terms of irradiation of light in the refracting media of the eye, we believe that an important component of the irradiation occurs within the substance of the optic papilla, which is composed of relatively unpigmented protoplasm, a sort of fibrous translucent jelly. That such irradiation occurs is indicated by subjective observation. When Helson presented his subjects with two squares one above the other, the description was of an upright rectangle. He also says that any light tends to spread throughout the whole optic disk.[8]

The subjective reports in our study agree with those of Helson, Scofield, and DeSilva and Weber on many points. Some of these as well as some differences may be pointed out. The most striking agreement is in the matter of diffuseness of stimuli. Helson says that the most typical phenomenon is a bright central core with a diffuse halo. Scofield's subjects spoke of an indefinite haze, DeSilva's of a blur of light—a diffuse haziness. Helson was able to obliterate the visible core of the stimulus by covering the center of the source light with a card. Our results do not entirely agree with this in that our subjects often saw a core in response to Fig. 1, which is constructed with a card over the center. The observation of concentric rings we do not consider a crucial test when the rings are of different color. At any rate only 2 of Helson's subjects (exclusive of himself) observed this and DeSilva and Weber failed to confirm it. We exclude the experimenter because Helson's data show that the knowledge of the forms of stimuli greatly enhance the perception of their forms.

[8] *Cf.* Helson, pp. 386, 391, f., 370.

In our opinion we cannot agree that a slight amount of form perception in the blind spot is conclusive evidence against the irradiation theory, since this slight amount may be due to slight loss of ocular control, or to differential irradiation throughout the optic disk, according to the principles involved in our account of the perception of a square in the blind spot. Very little stress should be laid upon such reports by Helson's subjects as "at the very first there was a suspicion of form," especially when the subject is unable to identify the form, because although the particular forms were unknown to the subject, he had been instructed to report on form, and also because the subject's knowledge of the experimenter's attitude toward perception in the blind spot may have favored such reports. An interesting indication that the experimenter may be important in an experiment is the fact that Helson's subjects saw in the blind spot the disk of color inside the ring of different color while none of DeSilva's subjects could see both colors at the same time.

Helson's failure to extend into the blind spot a light gradient set up from without is probably due not to the inadequacy of irradiation but to the strong contrast with the portion of the gradient outside the blind spot in accordance with the principles of contrast set forth by DeSilva. Other phenomena would probably also fail if strong lights were shown near the blind spot. In short, the light gradient fails under these conditions because it violates Helson's own law which states that in order to be visible the illumination on the blind spot must usually be greater than on the surrounding field. The operation and probable explanation of this law is illustrated by an exceedingly clever device of DeSilva and Weber. They caused a ring of light to change in size so as to stimulate now the very edges of the optic disk and now a larger ring on the retina, leaving the edges of the disk dark. With the edges of the optic disk lighted all around, the blind spot looked light, while with the larger surrounding ring stimulated and these edges dark, the blind spot looked dark. The nature of the sensory quality upon the blind spot is thus seen to be determined by the nature of the stimulation (with

due regard for contrast) of those receptors nearest the edge of the optic disk. This would also be true in the case of irradiation of light from *within* the optic disk. To return to the gradient, the reason that irradiation of light *into* the optic disk produces no vision is simply because that area is insensitive.

The totalizing and integrating action of receptors in the vicinity of the optic disk as presented by DeSilva seems to assist the physical irradiation in producing the phenomena observed in the blind spot. This is an example in which a type of Gestalt point of view may be useful. But the acceptance of the totalizing theory should not be used as a reason for avoiding further attempts to study and analyze the totalizing process, *i.e.* to reduce it to simpler terms in accordance with the doctrine of science. It may be noted for the mere purpose of classification that although both theories emerged from a Gestalt laboratory, DeSilva's is a Gestalt theory while Helson's is thoroughly mechanistic in accordance with the atomistic 'constancy hypothesis' which Gestalt psychologists condemn.

Summary

Two stimuli were designed which, by casting light upon mutually exclusive areas of the optic disk, would favor form discrimination as much as possible if the optic disk is sensitive to light, and which at the same time, by irradiating light uniformly in all directions, both of them in the same manner, would disfavor discrimination if such must depend on the retinal elements surrounding the optic disk. If form discrimination in the blind spot is due to the sensitivity of the optic disk, the uniformity and similarity in irradiation would not be unfavorable to such discrimination. The subjects' reports show that (1) there were some differences between the stimuli in brightness, size, definiteness, and degree of concentration (2) these differences were mostly small, (3) they were all in the direction opposed to what would be expected on the basis of the sensitive disk theory, (4) they were not incompatible with the irradiation theory, (5) the stimuli were not discriminated

on the basis of form, and (6) they could not be identified or differentially recognized when shown to the subjects in direct vision after having been shown repeatedly in the blind spot. We conclude that the sensitivity of the optic disk is neither sufficient nor necessary to account for what vision there is of objects in the blind spot.

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THE RELATIONSHIP BETWEEN 'SPEED' AND 'ALTITUDE'

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INTRODUCTION

An individual's altitude [1] score in a particular type of task has been defined by Thorndike as indicating the difficulty level he can reach in a series of problems of increasing difficulty, when no time restrictions are made. The question has frequently been raised as to the relationship between speed and altitude. If these are independent functions, the persons who reach the highest ability levels will not necessarily be 'quick' in mental tasks. But if speed and altitude involve a community of function, persons who are 'quick' will also be proficient in solving complex mental problems when no restriction is placed on time. This latter would be the case if speed in connection-making or speed in establishing patterns were actually an important contributing factor to the abilities represented in altitude scores. It would also be the case if altitude ability were an important factor in speed, the rate of problem solving being affected by the number of available connections.

Two methods have seemed applicable to the study of this problem: (1) the correlation of simple, discriminatory or serial reaction time (speed) with mental test scores (altitude); (2) the correlation of mental test scores under restricted time (speed) with test scores under expanded time (altitude). Neither of these methods has given clear cut results, for in the first case the measure of altitude has not usually been free

[1] In other studies on this problem, various terms have been used to designate the variable we are here calling altitude. 'Power,' for example, is frequently employed with approximately the same meaning. We are avoiding this term, however, because in the conventional physical definition 'power' includes a time factor (rate of work), a connotation which we wish to avoid.

from speed, and in the second case the measure of speed has included altitude. A refinement of this latter method has consisted in measuring the speed of mental test performance with accuracy constant (*i.e.*, timing the individual items on which no errors are made). In this way a speed measure is obtained which can be correlated with other speed measures in simpler functions, or with altitude scores. While this is a promising technique, it is exacting as to laboratory time and has not yet been applied where the criteria of an adequately large sample and a satisfactory measure of altitude were simultaneously met. The argument has rested usually upon the intercorrelation of speed scores in a variety of tests.

Hypotheses Applicable to the Present Experiment

The technique employed in this experiment was originated by Jones and given a preliminary trial by him in 1928 (see footnote, p. 102). This technique will be more fully described later. In brief, the tests of speed were given under conditions which involved the exposure on a screen of simple narrative descriptive material; the exposure times were experimentally varied by means of a projection apparatus. After seeing this material, the subjects were given completion tests designed to test their immediate recall of what had been observed. The same amount and the same kind of material was presented at each of four speeds. Scores on the completion tests were correlated (for each exposure rate) with altitude scores from a difficult completion test given under unrestricted time limits.

In subsequent pages the tests involving experimentally varied exposure rates will be referred to as 'time variable tests,' or '*TV* tests.' No assumption is made that these are 'pure' speed tests, that is, that they involve no range in difficulty. Any mental speed test must necessarily be constructed at some level of altitude. It is true that in the present instance an attempt was made to select material which seemed *a priori* to involve no range of difficulty for the subjects in question. But in this as in other investigations there is reason to believe that the individual items have actually varied over a range of altitude levels.

If all the items in a speed test were equally easy for all subjects, then the correlation between time on these items and altitude scores would give a direct measure of the relation between speed and altitude, *i.e.*, difficulty would have been held absolutely constant in the tests of speed. Such a control of altitude factors would seem almost impossible to achieve experimentally in mental test material. The procedure pursued in this experiment offers a means of avoiding this dilemma. The tests were so constructed that the material presented at each speed offered comparable ranges in difficulty. That is, though difficulty is not held constant in these speed tests, the amount of variation in difficulty is the same for all speeds. Hence in our results *we will not be concerned with the absolute magnitude of the correlations, but with changes in the correlations of the TV tests with altitude as the speed is experimentally varied.*

Under these experimental conditions, if there were no community of function between speed and altitude, we would expect the true correlations between altitude scores and *TV* scores to be all of the same magnitude over a considerable range of speed.[2]

On the other hand, if there were some community of function between speed and altitude, we would expect the correlations to change in some systematic way as we experimentally changed the speed. If TV_1 were a very slow speed, we would expect little if any positive correlation to be due to the community of function between speed and altitude, but largely to other factors common to both tests. But at TV_2, TV_3, etc. in addition to the factors common to test TV_1 and the altitude test, we would have other factors operating to increase the correlation, namely, some of those factors which speed and altitude have in common.[3]

[2] The correlation would of course drop to zero in the case of exposure times so short as to permit no one to read any of the material, or so long as to permit everyone to memorize the material verbatim.

[3] Certain assumptions are made here with regard to the speed ability, which should be stated explicitly. Our procedure assumes that each individual is able to adapt to increased speed conditions, up to a certain point; after that point his efforts result in failure. If we could use an infinite series of speed tests, it would be possible to discover the maximum speed of each individual for the particular function under

The Measurement of 'Altitude'

In the present investigation, altitude scores were obtained through the use of the completion elements of Thorndike's *CAVD* tests, levels *M*, *N*, *O*, *P* and *Q*. This test is designed to have a difficulty range for college students such that no one can do all of the items correctly though allowed unlimited time. The subjects were instructed to "write words on the dotted lines so as to make the whole sentence true and sensible"; they were told that their scores on this test would depend on the number of completions correctly made, and would be in no way affected by the rate of performance or by the time of finishing; all students were required to remain in the laboratory until the end of a 90 minute period, which was sufficiently long so that at the end of this time no one felt that he could appreciably improve his score by further work.

The Time Variable Tests

Previous evidence [4] has indicated that if any relation can be shown between speed and altitude, it will occur in the case of similar rather than of dissimilar functions. Accordingly, in our tests of speed we employed a verbal completion function similar in form to the altitude test.

a. Exposure Material

Twelve units or paragraphs of approximately 90 words each were constructed in such a way as to (1) present uniformly simple and interesting material within the comprehension of all subjects, (2) provide a wide variation of content, (3) provide for each unit, approximately 25 completions, when arranged with blanks in the form of a completion test.[5] Each of these selections was put on motion picture film, four

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consideration. Since this is not feasible, we select a certain number of speeds, so spaced as to exposure time that we are tapping in at a few of the different zones of this hypothesized speed ability. We further assume that, if there is some community of function between speed and altitude, the individual who adapts more satisfactorily to the faster speeds will tend to get a higher altitude score.

[4] The reader is referred to Tryon, C. McC., On the nature of 'speed' and its relation to other variables. J. General Psychol. 1932, **7**, No. 2.

[5] A sample unit is given herewith. In the test the paragraph was reproduced, with the italicized words omitted. These words were of course not italicized in the original presentation:

During the *night* the
sea lizards lie in
burrows or in deep *lava*

frames to a unit, and approximately 23 words to a frame. In the experimental situations, this material was projected on a screen at four exposure rates, the length of exposure being controlled in a projecting tachistoscope. Three units were used at each of the four speeds. Comparison of results from the four exposure rates must assume equivalence of the units in intrinsic difficulty, and must also assume equivalence in practice effects. To meet the first of these requirements, the 12 units were ranked as to difficulty by ten graduate students in psychology; differences in difficulty were extremely small, and were equated by dividing the units into four groups (one for each speed) of equal average difficulty according to this criterion. The second requirement was met by scheduling the units so that the same average position in series would be obtained by the units at each speed—thus the three units at the slowest speed were shown 1st, 6th and 12th in series; the three units at the next speed were shown 3d, 5th and 10th in series, etc. Unless the practice curve were of unusual form, this would serve to eliminate differential practice effects—and probably also other differential effects pertaining to position in series (such as motivation, and fatigue).

b. The Exposure Rates

After some preliminary experimenting with time allowances, using a small group of graduate students, it was decided to let exposure time at the slowest speed approximate one word per second, and to decrease by geometric ratio the amount of exposure time for the other speeds.[6] This geometrical decrease was followed for the four speeds

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crevices; during their
leisure time by *day*
they bask in the sun.

When the *tide* is suitable,
they *graze* among the
seaweed, showing marked
preference for *quiet*
places. They tolerate
being *lifted* and *stroked*.

Their *reactions* are *few*.
They do not try to *bite*,
and if they *scratch*, it
is by *mistake*. They
clutch the *volcanic* rock
with *talon-like* claws.

When *excited*, they
inflate their *bodies*
and send out from
their *nostrils* a double
jet of *watery breath*.

[6] In his preliminary work Jones used three paragraphs of approximately 120 words each, presented on a screen by means of motion picture film and a motion picture projector. Three rates of speed were used (60, 40 and 25 seconds), the position in series of each test being shifted in different groups of students. Scores obtained under these speed conditions were correlated with Army Alpha scores already available for the subjects he was testing (100 elementary psychology students). Since the means

within practical limits. (Since the material had to be presented by 'frames' of film, and since each frame had to be timed, the ratios were varied sufficiently to avoid split seconds.) TV_3 and particularly TV_4 appeared to the subjects to be extremely fast, and to place a very high premium upon a 'triggered' inspection of the material. The following are the lengths of exposure for each.

Speed	One Test (three units) Approx. 270 Words	Unit Approx. 90 Words	One Frame Approx. 23 Words
TV_1 (slow)	300 seconds	100 seconds	25 seconds
TV_2	156	52	13
TV_3	84	28	7
TV_4 (fast)	48	16	4

c. The Test Material

The completion blanks, bound into a booklet, repeated each paragraph thrown on the screen, except that in each case 24 of the words were left blank. Each test was on a separate page, with instruction against turning the pages prior to the tachistoscopic exposure. The subjects were told to fill in the words, and that good substitutes would receive credit if the exact words were not recalled. An effort was made to arrange the sequence of ideas in such a way that one element or idea did not inevitably lead on to the next. In other words, completions were sought which would depend on the recall of the specifically learned material presented on the screen, and not on the ability to infer what ought to come next. While the completion blanks repeated the exact wording of the paragraph presented on the screen (with certain words omitted), they did not retain the same typographical form, that is, the number of words to the line and the number of lines. This precaution was taken to prevent subjects from filling in isolated words remembered simply on the basis of position without comprehension of the sense of the sentence or paragraph.

d. The Experimental Procedure

The same procedure was followed for each test: The subjects were warned of the amount of time allowed for presentation; the lights were turned out, a reading unit was presented on the screen, in four successive frames; then the lights were turned on and instructions given to turn the page and begin. As much time as seemed necessary was allowed for the recall process. In no case did any individual take more than four minutes to supply all that he could recall. Hence these tests *were concerned with variations in the amount of time for reading comprehension and not with variations in the amount of time for recall.*

e. Scoring

For each test, the items based on the first frame were not scored, since it was thought that greater reliability could be obtained by utilizing the first exposure as practice material, permitting an adjustment of the subjects to the speed and general

and standard deviations of the speed scores were all of approximately the same magnitude, and the correlations between these tests and Army Alpha were of the same order (.65 corrected for attenuation), Jones concluded that greater extremes in exposure time in the tests of speed and a more adequate measure of altitude should be used. Further, reliability coefficients of the order .65 for each of these speed tests pointed to the desirability of increasing the amount of material at each exposure rate.

situation, and eliminating disturbances due to wandering attention or to imperfect visual fixation. Emotional interferences are probably also more operative on the first frame. In the test material based on the remaining three frames (in each unit) two points were given for each completion which reproduced the exact word in the original paragraph, or which was a good equivalent of the original word. Less adequate synonyms or completions indicating a rather hazy recollection of the presented material were scored one point each. Whenever a new word was given credit, it was added to the key and given the same credit in remaining tests.

The maximum scores attainable on the tests were as follows:

CAVD completion element..... 229
Each of the four speeds..... 120 (40 for each unit)

Sampling

Elementary psychology students were used as subjects. A total of 116 subjects took all of the tests at meetings of the laboratory sections. There were five of these sections varying in size from about 15 students to about 35 students. Exactly the same instructions and procedure were used in each of the five sections, and so far as could be observed a uniformly high level of interest and coöperation was maintained in all sections.

All tests were group tests. The *TV* tests were given during the first part of the period; these took one hour and ten minutes. The subjects were then given a rest period of ten minutes, and the last hour and one-half of the three hour period was allowed for the altitude test.

Results

The intercorrelations between the *TV* and altitude tests (both crude and corrected for attenuation), reliability coefficients, means, standard deviations, and the probable errors of these constants are set forth in the following tables and graphs.

In Fig. 1 are shown the distributions of the total scores at each of the four speeds; namely TV_1 300 seconds exposure time; TV_2 156 seconds; TV_3 84 seconds; and TV_4 48 seconds. (In all of the following tables and graphs, *A* represents the altitude test, TV_1 the slowest speed, TV_2 the next faster, TV_3 the next faster and TV_4 the fastest.) The means and standard deviations for these tests together with probable errors are shown in Table I.

TABLE I

	Mean	S.D.
A	161.6 ± 2.0	31.2 ± 1.4
TV_1	76.8 ± 1.0	17.5 ± .8
TV_2	77.4 ± 1.2	15.2 ± .7
TV_3	62.8 ± 1.4	19.2 ± .9
TV_4	37.6 ± .9	14.9 ± .7

It may be noted from Fig. 1 that the distributions of scores for TV_1 and TV_2 are very similar. The effect of decreased exposure time becomes apparent in TV_3; the distribution is moved down on the scale significantly. This exposure rate tends to spread the 116 individuals slightly better than do the

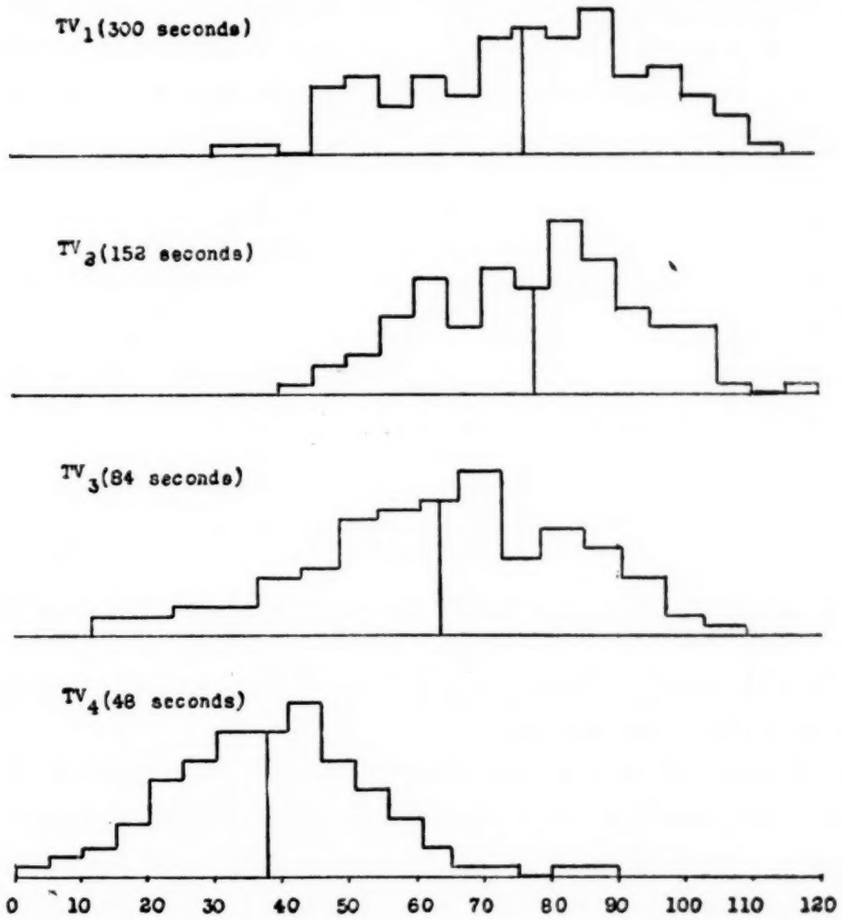

FIG. 1. Distribution of scores on four time variable tests.

other three tests (see Table I). At the fastest speed, TV_4, the distribution of scores is of the same nature as those for tests at the other exposure rates; the standard deviation is slightly smaller though not significantly different statistically. Had a still faster rate of exposure been used, it probably would have resulted in a skewed distribution.

We find that the decrease in amount of reading time according to geometrical progression is *not* consistently accompanied by a decrease in average completion score (see Fig. 2). There is no difference between mean scores when the

amount of reading time is decreased from 300 seconds (TV_1) to approximately one-half of that, namely 156 seconds (TV_2). After this point geometrical decrease of time tends to result in a geometrical decrease in the means. The number of speeds

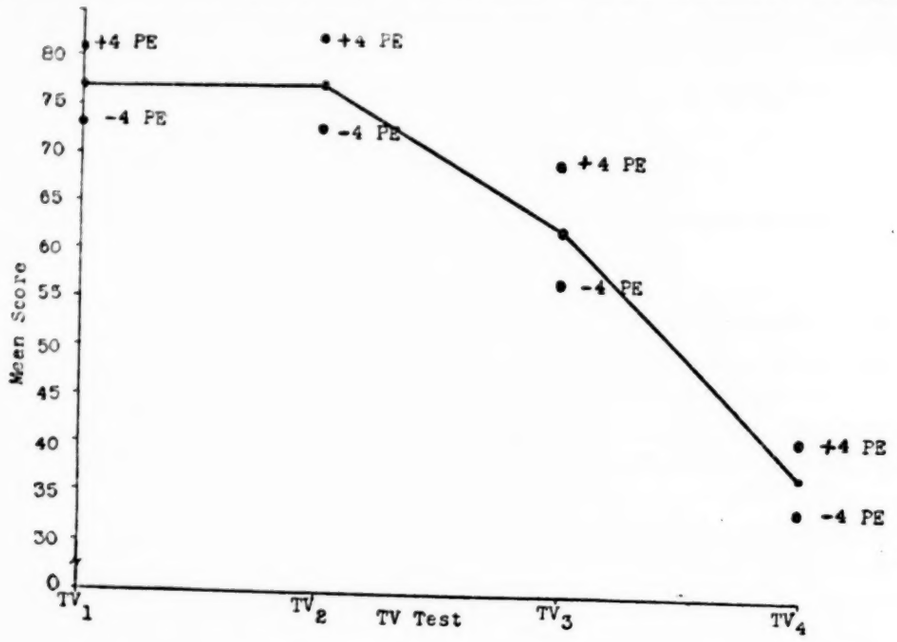

FIG. 2. Change in mean score with change in exposure rate (Data from Table I).

used in this study, however, is too small to draw conclusions regarding this relationship.

In Table II are given the reliability coefficients for the altitude test and for each of the four time variable tests. The reliability coefficient for the altitude test was computed by applying the Spearman-Brown formula to the correlation between the scores on odd sentences vs. even sentences in the test. The reliability coefficients for the speed tests were computed by applying the Spearman-Brown formula to the

TABLE II

RELIABILITY COEFFICIENTS [7]

A	$.94 \pm .01$
TV_1	$.87 \pm .02$
TV_2	$.84 \pm .02$
TV_3	$.92 \pm .01$
TV_4	$.84 \pm .02$

[7] Probable errors of reliability coefficients obtained from Cureton, E. E., and Dunlap, J. W., Nomograph for estimating a reliability coefficient by the Spearman-Brown formula and for computing its probable error, *J. Educ. Psychol.*, 1930, **21**, 68–69.

correlation between scores on odd vs. even lines in the completion forms.

The raw correlations between scores on the altitude test and scores on each of the speed tests, and intercorrelations between each of the speeds are shown in Table III. In Table IV these same correlations are corrected for attenuation by the conventional formula, namely, the raw correlation divided by the geometric mean of the reliability coefficients.

TABLE III

RAW CORRELATIONS BETWEEN THE ALTITUDE TEST AND EACH OF THE *TV* TESTS, AND INTERCORRELATIONS BETWEEN THE *TV* TESTS

	A	TV_1	TV_2	TV_3
TV_1	.50 ± .05			
TV_2	.37 ± .05	.70 ± .03		
TV_3	.43 ± .05	.73 ± .03	.72 ± .03	
TV_4	.51 ± .05	.66 ± .03	.63 ± .04	.75 ± .03

TABLE IV

CORRELATIONS (CORRECTED FOR ATTENUATION) BETWEEN THE ALTITUDE TEST AND EACH OF THE *TV* TESTS, AND CORRECTED INTERCORRELATIONS BETWEEN THE *TV* TESTS [8]

	A	TV_1	TV_2	TV_3
TV_1	.55 ± .05			
TV_2	.42 ± .06	.82 ± .05		
TV_3	.47 ± .05	.82 ± .03	.81 ± .03	
TV_4	.57 ± .05	.77 ± .04	.75 ± .04	.86 ± .03

[8] Probable errors for corrected coefficients of correlation computed by formula 161, p. 209, Kelley, T. L., Statistical Method, The Macmillan Co., 1924.

From Table IV we may note that the correlation of the slowest test with altitude is approximately the same as that of the fastest test (.55 and .57 respectively, corrected). The correlation of altitude with the second test is the lowest of the four, and after this speed the correlations tend to increase with increased speed according to the hypothesis mentioned earlier, which postulated some community of function between the two abilities. However, these differences between the correlations of the different speeds with altitude could easily occur

by chance. Graphic evidence that these differences may be due to unsystematic factors is given in Fig. 3, where plus and minus four probable errors are indicated for the corrected correlations.[9]

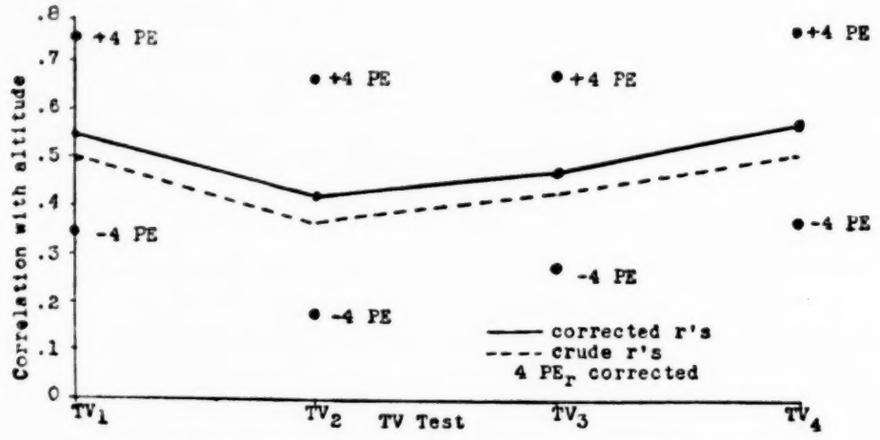

FIG. 3. Correlation of *TV* test scores with altitude test scores (Data from Tables III and IV).

Note that the intercorrelations between the four time variable tests in Table III (raw) and Table IV (corrected) are higher than the correlations between these tests and the altitude test and that they are, among themselves, approximately of the same magnitude, ranging from .75 to .86 (corrected for attenuation). There seems to be a slight tendency, however for the correlations to increase systematically as the amounts of exposure time become more alike. For example, $r_{(TV)1(TV)4}$ is lower than $r_{(TV)1(TV)3}$ and $r_{(TV)1(TV)2}$, and $r_{(TV)2(TV)4}$ is lower than $r_{(TV)2(TV)3}$ and $r_{(TV)2(TV)1}$, and so on. But here again the differences are very slight and certainly not statistically significant.

If there were great inherent variations in difficulty in the test material from speed to speed, we would expect it not only to obscure real differences in correlations between the several variables (speed with speed and speed with altitude), but also to obscure systematic changes between the mean values. If

[9] The greatest difference between crude correlations ($r_{A(TV)_2} - r_{A(TV)_4}$) was less than three times the P.E.$_{diff.}$ calculated by the formula,

$$\text{P.E.}_{r_{A(TV)_2}-r_{A(TV)_4}} = .6745\sqrt{\sigma^2_{r_{A(TV)_2}} + \sigma^2_{r_{A(TV)_4}} - 2r_{r_{A(TV)_2}r_{A(TV)_4}}\sigma_{r_{A(TV)_2}}\sigma_{r_{A(TV)_4}}},$$

where $r_{r_{A(TV)_2}r_{A(TV)_4}}$ is computed from formula 128, Kelley, T. L., page 179, *op. cit.*

we compare Fig. 2 and Fig. 3 we note that while the mean scores of the speed tests (beginning with TV_2) are very markedly affected by change in speed, these changes in mean score are not accompanied by concomitant changes in correlation of these speeds with altitude. Further, the intercorrelations obtained between the speeds are likewise unaffected by the great changes in mean score (see tables III and IV). In other words, individuals tend to keep their same rankings in spite of great changes in mean score.

Analysis of the Individual Tests

An analysis of the twelve separate units was next made to discover whether there might be certain factors such as practice effect, variation in difficulty, etc. which were obscuring a systematic trend in the relation between the scores on the speed tests and those on the altitude test.

In the following discussion, the units given at the same speed are designated *a*, *b* and *c*. Thus, for example $TV_{1_a}TV_{1_b}TV_{1_c}$ are the three units given at slow speed. Test *a* in each speed was given first in series for its respective speed, *b* second, and *c* third. Test TV_{1_a} was presented first in the total series, TV_{1_b} sixth, and TV_{1_c} twelfth. Table V gives the

TABLE V

Statistical Constants for the 12 Units

	M	S.D.	Reliability Coef.	*r* with Altitude	
				Raw	Corrected
TV_{1_a}	22.9 ± .43	6.9 ± .31	.69 ± .04	.36 ± .05	.44 ± .06
TV_{1_b}	24.4 ± .50	7.9 ± .35	.80 ± .03	.51 ± .05	.59 ± .05
TV_{1_c}	30.0 ± .43	6.9 ± .31	.77 ± .03	.32 ± .06	.38 ± .06
TV_{2_a}	28.1 ± .38	6.0 ± .27	.70 ± .04	.35 ± .06	.43 ± .07
TV_{2_b}	28.7 ± .35	5.7 ± .25	.70 ± .04	.27 ± .06	.45 ± .10
TV_{2_c}	24.0 ± .46	7.4 ± .33	.75 ± .03	.29 ± .06	.34 ± .07
TV_{3_a}	22.2 ± .52	8.4 ± .37	.84 ± .02	.11 ± .06	.12 ± .07
TV_{3_b}	24.8 ± .42	6.7 ± .30	.76 ± .03	.25 ± .06	.30 ± .07
TV_{3_c}	24.7 ± .54	8.7 ± .38	.84 ± .02	.31 ± .06	.35 ± .06
TV_{4_a}	15.0 ± .39	6.2 ± .28	.64 ± .05	.51 ± .05	.66 ± .06
TV_{4_b}	15.8 ± .43	6.9 ± .31	.77 ± .03	.47 ± .05	.55 ± .05
TV_{4_c}	13.0 ± .37	5.9 ± .26	.69 ± .04	.19 ± .06	.24 ± .07

means, standard deviations, reliability coefficients and correlations of each test with altitude (both raw and corrected).

There is no evidence indicating that position in series affects the scores, though this factor might to some extent be obscured by inherent difficulty of certain of the tests. There seems to be little or no relationship between the difficulty of the unit as indicated by mean score and the amount of correlation with altitude. In general the variation among the correlations of the twelve paragraphs with altitude seems to show no systematic trend. The probable errors of these correlations indicate that for the most part they are insignificantly different from each other.

Interpretation

If there is any relationship between speed of comprehending simple narrative-descriptive material and the ability to make completions in complex material, this relationship is not revealed in the present study. When the amount of exposure time was varied from 300 seconds to 48 seconds for the same amount and the same kind of material, there was no significant systematic change in the correlation between response to this kind of material and response to the *CAVD* material. When a simple paragraph is given at the slowest speed, in which speed of comprehension would seem to be a negligible factor (all individuals having time to read and reread the simple material), the correlation with altitude is not significantly different from that found when a simple paragraph is exposed for so short an interval that speed of reading comprehension would be an important factor.

Since the correlation of each of the time variable tests with altitude is of the order .50, the question arises, what are the common factors which produce this correlation? In considering this question, we must note further that the intercorrelations between the four different *TV* tests are high, of the order .80, and that there is only slight evidence that there is a systematic tendency for the different speeds to increase in correlation as the exposure times become more similar.

Any tests such as those used here to measure speed could

not be entirely free of factors also common to the altitude test, *e.g.*, factors such as comprehension, retention, and so on. Possibly altitude ability operates to some extent in the time variable tests, in the act of supplementing recall; thus, if a certain number of completions have been filled in through recall of the actually observed material, some of the remaining completions may then be inferred on the basis of a 'completion ability,' and a general acquaintance with the context. In Fig. 4 is shown a schematic presentation which attempts to

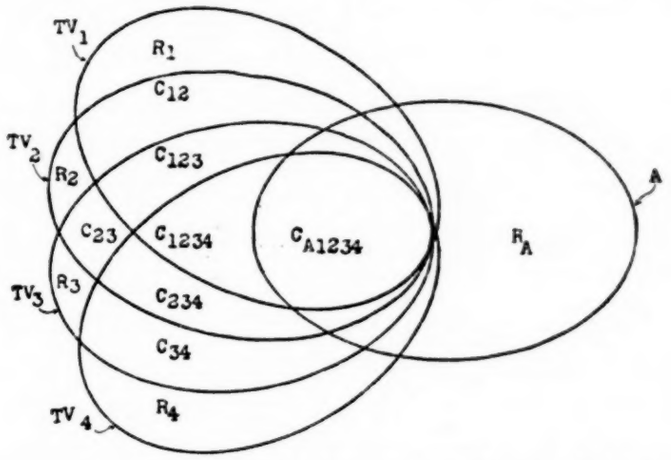

FIG. 4. Diagrammatic representation of the postulated factorial relationships between the variables.

account for the correlations found between the tests used. (No attempt is made to show the exact percentage determination.) According to this diagram, the four *TV* tests have a certain group of factors in common with altitude and with each other (C_{A1234}) which would account for the fact that the correlations of each *TV* test with altitude are insignificantly different from each other and are of the order .50.

The schematic diagram shown in Fig. 4 would also account for the high degree of relationship between the several *TV* tests (of the order .80). In addition to the factors which these tests of speed had in common with the altitude test, there were other factors common to them (C_{1234}) but not common to the altitude test. Some of these might be ability to adapt the vision to screen material, capacity for perseveration or for instant recall, and so on.

Finally the schematic diagram shown in Fig. 4 would

account for the possible tendency for adjacent speeds to correlate higher than more remote ones. (It was pointed out above, that though these differences are very slight and not statistically significant, the trend is fairly consistent.) For example, TV_4 has more factors in common with TV_3 than with TV_2 and TV_1, namely C_{34}. Speed of visual fixation and speed of word perception might be common factors tending to boost (but to a slight degree) the correlations between adjacent speeds.

Residual factors (R) would account for the fact that none of the correlations are unity. In the case of the altitude test, we find a large dosage of residual factors (R_A), since the correlations with the speed tests are of the order of .50. This R_A may represent factors entering into sheer comprehension of complex material which were held constant in the tests of speed. The dosage of residual factors in the different TV tests (R_1, R_2, R_3, and R_4) is much smaller. These may represent such factors as differences in *familiarity* with the type of narrative-descriptive material presented in the twelve units, differences in interest in the type of material, and so on.

In presenting this interpretation, the writers are aware that other structures could be offered to account for the correlations obtained. This has been selected for discussion since it seems to be the simplest, since it is consistent with the correlational data, and since it is consistent with an *a priori* psychological analysis of the variables.

In conclusion we might say that wide variation in the amount of exposure time, which would put a variable premium on speed of reading comprehension, seemed to be a negligible factor in both the intercorrelations between the tests given at different exposure times and between these tests and the altitude test. The ability to solve complex problems (completion element of *CAVD*) appeared unrelated to speed of reading and comprehending simple material, since the altitude test and the tests given under widely different speed conditions correlated to the same degree. Further, individual differences in the ability to do a single kind of comparatively simple task are relatively unaffected by speed (variation in amount of

exposure time), since individuals still tended to keep their rank order to a high degree (.80) when the amount of exposure time was reduced approximately six times.

Summary

1. The purpose of this investigation was to study the relation between speed and altitude in intellectual performance.

2. As an altitude test, the completion element of the *CAVD* was used, at levels standardized for college students. This was administered without time restrictions.

3. A series of 'time-variable' (*TV*) tests were constructed, consisting of completion-recall units. These were based upon simple reading material projected on a screen at four exposure rates (varying from a slow presentation at approximately one word per second to a very rapid presentation at approximately six words per second).

4. In a group of 116 college students, the average immediate recall score in the *TV* tests was *64* percent of the total number of items at the slowest speed, dropping to *31* percent at the most rapid speed.

5. The reliability coefficients for all of the tests were high: .94 for the altitude test and between .84 and .92 for each time variable test.

6. It was assumed that if there were a community of function between speed and altitude in mental performance, this would be expressed in an increasing correlation between altitude and the *TV* tests based on the more rapid exposures. The correlations, however, between altitude and each of the four *TV* tests were insignificantly different from each other, and of the order .50. When comparisons were based on the correlations corrected for attenuation, similar results were obtained.

7. Analysis of each of the sub-tests or units at each speed indicated that these twelve units did not significantly differ from each other in difficulty or in effect of practice, and that the failure of the total tests at each speed to show systematic trends in correlation could not be ascribed to flagrant variation in the difficulty and practice effects in the sub-tests.

8. The intercorrelations between *TV* tests were of the order of .80 corrected for attenuation (.70 raw). There was a consistent tendency for adjacent speeds to correlate higher with each other than with more remote ones, although these differences were very slight and not statistically significant.

9. The results were interpreted as indicating

a. that speed of mental processes was a negligible factor in determining the correlations between the test of altitude and the tests given under widely varying time restrictions.

b. that speed exerted only a slight influence upon the intercorrelations of the *TV* tests given under widely varying time allowance.

10. For the practical purposes of predicting altitude ability, this study indicates that for fairly simple material of a completion nature, it makes little difference, within wide limits, whether or not time is restricted.

11. The present study gives no support to those theories which maintain that mental capability, measured either with or without reference to time, is contingent upon the speed of mental processes.

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AFFECTIVE, VOLITIONAL AND GALVANIC FACTORS IN LEARNING

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Problem

The purpose of this investigation was to study the interrelationships of the following factors: (1) the affective value of the materials used in learning; (2) the volitional attitude assumed during learning; (3) the efficiency of learning; and (4) the galvanic skin deflections elicited upon the presentation of the materials.

The general method of approach to this problem was as follows: Subjects were presented with lists of words that contained paired associates of a pleasant, unpleasant, and indifferent character. They were instructed to assume either an active or a passive attitude while learning a list. Each list was presented but once, and recall followed immediately after presentation. The efficiency of learning was measured in terms of the number of items recalled and the speed of recall. Measures of the maximum magnitudes of the galvanic skin reflex deflections elicited during learning were obtained.

Materials, Procedure, and Apparatus

The materials used in this experiment differ from those used in other investigations in that the affective value of the paired associates was judged as a unit rather than as two words individually. In this way a greater range of affective value was secured. The word 'sores' in itself is unpleasant; combined with the verb 'suck' to form the paired associate 'suck sores,' the unpleasant tone is greatly intensified. The word 'sweetheart' alone has a pleasant connotation; combined with the verb 'embrace' in the paired associate 'embrace sweetheart' the pleasantness is enhanced. Through the use of such combinations the affective value was accumulative and stronger than if single words had been used.

Paired words, such as those cited above, were submitted to several students in Psychology in order to secure their judgments as to the affective value of each pair. From those upon which there was the greatest unanimity of judgment as to their unpleasant, pleasant, or indifferent character, was selected a number sufficient for the construction of 96 lists of 12 pairs each. In all, there were 1152 pairs, 384 each of pleasant, unpleasant, and indifferently toned paired associates.

The factor of position was eliminated by varying systematically the arrangement within a list so that each of the three types of paired associates occupied every position in a successive series of 3 lists, and by having the order of presentation for recall differ from that during learning.

Twenty subjects, students in elementary and advanced courses in Psychology, took part in the experiment. Each subject reported for 24 experimental sittings, at intervals, approximately, of 24 hours. Each sitting lasted from 20 to 45 minutes. Each subject was presented with four lists in a single session.

The regular experimental series was preceded by two practice sessions for the purpose of ascertaining the optimum interval of exposure for the lists, and of accustoming the subjects to the conditions of the experiment.

All the 20 subjects learned the series of 96 lists and in the same order. Each list was presented but once. The amount learned during the single presentation was then measured by testing for immediate recall. The number of items correctly recalled and the times of recall were recorded.

The subjects were instructed to assume, while learning a list, either of two attitudes—an active or a passive attitude. No suggestion was made as to which of the two attitudes might be conducive to more efficient learning. The subjects were warned at the beginning of an experimental session to maintain the prescribed attitude throughout the presentation of a list. Practice effect was equalized for the two attitudes by counterbalanced order: The two attitudes were alternated from list to list. Of the 4 lists learned in a single session, lists 1 and 3 were learned under one attitude and lists 2 and 4

under the other. Moreover, half the subjects began a series with one attitude, and the other half began the same series of 4 lists with the other. The conditions were reversed on the following day: those subjects who had begun the previous day's series with the passive attitude now began the series with the active attitude and vice versa.

The time of exposure of each pair in a list was two and one quarter seconds. An interval of two and one quarter seconds was allowed to elapse following the exposure so as to allow for the subsiding of any galvanic skin reflex deflections that may have been elicited.

The lists were exposed by means of a Hull Memory Drum, which controls automatically the time of exposure and the interval elapsing between two successive exposures. The drum was mounted on the table in front of the subject at a distance convenient for clear vision.

The times of recall were recorded by means of a stop watch showing fifths of a second.

The galvanic skin reflex deflections elicited by the paired associates during their presentation were obtained by placing the subject in circuit with the Hathaway Psychogalvanic Response Apparatus, the electrodes being bound against the palm of the right hand. This apparatus was mounted on the table to the right of the subject and out of the line of his direct vision.

Through the use of alternating current as electrode potential, the Hathaway apparatus eliminates, professedly, polarization, even with the use of dry electrodes. Through the use of a milliammeter, in place of the usual galvanometer, immediate recording of the magnitude of the deflections in terms of the excursions of the marker to the right on the milliammeter scale is made possible. Readings are in hundredths of a milliammeter.

Only those galvanic skin reflex deflections elicited upon presentation of the materials during learning were recorded. It was not feasible to record them during recall inasmuch as the recording of the number of items correctly recalled and the times of recall occupied the experimenter's entire time.

It was not possible to attach either a kymographic or a photographic device to the apparatus. So, no attempt was made to record either the time elapsing before a deflection reached its maximum or the duration of a deflection. The measures of the deflections were invariably in terms of their maximum magnitude.

Part I. Influence of Affective Value of Materials upon Efficiency of Learning and Galvanic Skin Reflex Deflections

The results of this phase of the experiment are given in Tables I and II. Table I summarizes the findings with regard to the influence of the affective value of the materials upon the amount and speed of recall. Table II summarizes the findings with regard to the influence of the affective value of the materials upon the number and maximum magnitude of the galvanic skin reflex deflections elicited upon their presentation during learning. The results of the 20 subjects taking part in the experiment form the basis of the calculations.

TABLE I

Average Number of Items Recalled per List and Average Time per Item Recalled

Affective Value	No. per List	Aver. No. Recalled	σ (dis.)	Total No. Recalled	Aver. Time in Seconds	σ (dis.)
Pleasant	4	2.95	1.579	5655	1.87	1.093
Unpleasant	4	2.91	1.044	5590	1.89	1.114
Indifferent	4	2.92	1.065	5605	1.85	1.114

These figures indicate that the differences in the average number and average time of recall of pleasant, unpleasant, and indifferent items are quite small; a computation of the probable error of the differences showed that they are unreliable. We must conclude, therefore, that the effective value of the materials did not influence either the amount or speed of recall.

These results are in accord with the findings of Wohlgemuth (**25**) and of Gordon (**9**), who, however, did not measure

the speed of recall. They are not in agreement with the findings of others. An explanation for the variance of results may be sought in terms of the conditions of the several experiments as compared with those of this study. Peters (**16**), Peters and Nemecek (**17**), and Henderson (**11**), for example, found that unpleasantly toned experiences were recalled less often than either pleasantly or indifferently toned experiences; and Baxter, Yamada, and Washburn (**1**) found that a longer reaction time was required for recalling unpleasant experiences. These investigators used the method of directed recall. That is, they required their subjects either to react to a stimulus word with the memory of an actual personal experience or merely to list personal experiences and then to classify them with regard to their affective tone. In this manner, they practically approximated the conditions that held in Freud's investigations. At the same time, however, they left uncontrolled the interfering factors of the probable difference in the number of such experiences, in the intensity with which they were originally fixated and acquired, and in the relative recency or remoteness of their occurrence. They seem to tacitly postulate that pleasant, unpleasant and indifferent experiences occur with equal frequency and equal intensity, and that remote and recent experiences are recalled equally often. Also they did not obviate the possibility that their subjects may have actually recalled many unpleasant experiences, but were reluctant to admit them in either verbal or written statements. Further, and obviously, the experiences listed were not acquired originally with the intention of subsequent recall. It may be noted here that Wohlgemuth (**25**), using this same method of directed recall and controlling the interfering factors of the probable difference in recency or remoteness and number of pleasant and unpleasant experiences, obtained results with which the findings of the present investigation are in accord. In view of the variances in method of the several investigators, we are precluded from making direct comparisons with any study other than that of Gordon (**9**).

It may be observed that the figures of the three types of

TABLE II

AVERAGE NUMBER OF DEFLECTIONS ELICITED PER LIST AND AVERAGE MAXIMUM MAGNITUDE PER DEFLECTION ELICITED

Affective Value	Aver. No. Elicited per List	σ (dis.)	Total No. Elicited	Aver. Max. in Milliammeters per Deflection	σ (dis.)
Pleasant	.684	1.027	1315	.0297	.0182
Unpleasant	.596	.936	1145	.0268	.0194
Indifferent	.648	.968	1244	.0278	.0189

material are almost identical, showing no effect of the affective value of the materials upon either the frequency or maximum magnitude of the deflections elicited. A computation of the probable errors revealed no significant differences between the pleasant, unpleasant, and indifferent items. Washburn and Pisek (**24**) in Galvanometer Experiments with Revived Emotions as a Test of Emotional and Phlegmatic Temperaments obtained similar results.

PART II. INFLUENCE OF CONDITION OF LEARNING UPON EFFICIENCY OF LEARNING AND GALVANIC SKIN REFLEX DEFLECTIONS

This phase of the experiment was concerned with the influence of the active and passive attitudes assumed while learning a list upon the number of items recalled and the speed of their recall, and upon the number of deflections elicited during the presentation of the paired associates and their maximum magnitude. The results appear in Tables III, IV, V, VI, VII, and VIII.

TABLE III

AVERAGE NUMBER OF ITEMS RECALLED PER LIST AND AVERAGE TIME PER ITEM RECALLED

Learning Condition	No. per List	Aver. No. Recalled per List	σ (dis.)	Total No. Recalled	Aver. Time in Seconds	σ (dis.)
Active	12	10.01	1.687	9613	1.84	1.102
Passive	12	7.54	2.298	7237	1.92	1.109

These figures indicate a difference in the efficacy of the two conditions of learning. The active condition is shown to

be decidedly the better, as far as amount of recall is concerned, and somewhat better, as far as speed of recall is concerned. A computation of the probable errors of the differences with regard to amount showed them to be reliable. The reliability of the difference is indicated further by an inspection of the results for the individual subjects (see Table IV). A computation of the probable error of the difference with regard to speed showed it to be insignificant. Further indication of the probable statistical insignificance of this difference may be gained from an analysis of the results for the individual subjects (see Table V). We may conclude, then, that the active condition of learning facilitated the efficiency of learning when the measure of efficiency was the amount recalled. Earlier investigators, with the exception of Fukuya (**8**), obtained results with which those of the present experiment are in accord. However, even Fukuya's results indicate some

TABLE IV

AVERAGE NUMBER OF WORDS RECALLED PER LIST

Name	Active Attitude	Passive Attitude	Difference	
			More Gain with Active	Percent More with Active Attitude
Ca	9.25	7.56	1.69	22
Da	10.06	8.65	1.41	16
Fi	10.00	6.64	3.36	51
Fs	10.96	8.31	2.65	32
Ha	9.44	7.29	2.15	29
He	10.08	8.06	2.02	25
Ho	10.46	8.23	2.23	27
Hw	9.90	7.04	2.86	41
Is	9.96	7.19	2.77	39
Ma	8.60	5.29	3.31	62
Ob	10.11	7.63	2.48	33
Pa	10.48	8.17	2.31	28
Si	10.08	7.10	2.98	42
Sm	10.29	8.35	1.94	23
Su	10.11	8.15	1.96	24
Ue	10.36	8.27	2.09	25
Vo	9.35	7.15	2.20	31
Wh	10.11	7.27	2.84	39
Wm	10.17	6.83	3.34	50
Wo	10.54	7.58	2.96	38
Average	10.01	7.54	2.47	33

N—Individuals—48.
N—Group—960.

margin of superiority in favor of more efficient mental work when the active attitude was assumed, for he notes, that on the whole, the active attitude was more efficient for short periods of work and at the beginning of work.

TABLE V

AVERAGE TIME PER ITEM RECALLED
(In Seconds)

Name	Active Attitude	Passive Attitude	Difference	
			Longer with Active Attitude	Longer with Passive Attitude
Ca	2.18	2.32		.14
Da	1.79	1.84		.05
Fi	1.41	1.49		.08
Fs	1.89	2.17		.28
Ha	2.44	2.47		.03
He	1.47	1.48		.01
Ho	1.81	1.76	.05	
Hw	2.03	1.94	.09	
Is	1.47	1.67		.20
Ma	2.00	2.13		.13
Ob	1.96	1.90	.06	
Pa	1.45	1.65		.20
Si	1.80	1.81		.01
Sm	1.79	1.68	.11	
Su	1.76	1.62	.14	
Ue	1.95	1.97		.02
Vo	2.86	2.78	.08	
Wh	1.60	1.79		.19
Wm	1.78	2.39		.61
Wo	1.49	1.68		.19
Average	1.84	1.92		.08

N—Individuals—254 to 526.
N—Group—9613 (Active)—7237 (Passive).

The figures in Table VI reveal a difference between the influence of the two conditions of learning upon the frequency and extent of the galvanic skin reflex deflections elicited during

TABLE VI

AVERAGE NUMBER OF DEFLECTIONS ELICITED PER LIST AND AVERAGE MAXIMUM MAGNITUDE PER DEFLECTION ELICITED

Learning Condition	Aver. No. Elicited per List	σ (dis.)	Total No. Elicited	Aver. Max. in Milliammeters per Deflection	σ (dis.
Active	3.06	2.475	2936	.0302	.0229
Passive	.80	1.268	768	.0203	.0151

the presentation of the materials—galvanic skin reflex deflections were elicited more frequently under the active condition and their maximum magnitude was greater under this condition. A computation of the probable errors of these differences showed the differences to be reliable. Their reliability is further indicated by an inspection of the averages for the individual subjects (see Tables VII and VIII). Several earlier studies, not precisely concerned with this phase of the problem of our experiment, indicate that increased mental activity is accompanied by increased deflections of the galvanic skin reflex and thus, apparently, give indirect support to the findings of this study. Starch (**21**), for example, in an investigation of "Mental Processes and Concomitant Galvanometric Changes," found that mental activity accompanied by physical activity and also voluntary mental activity produce larger galvanic skin reflex deflections than does automatic mental activity. In an investigation of the supposed emotional significance of the psychogalvanic reflex, Brown (**5**) obtained only negative results, but was able to show that qualities which have an element of 'will' in the sense of consciously directed activity, correlated most highly with the galvanic reflex, and that those qualities having a predominantly emotional aspect all had low correlations with the reflex.

Part III. Influence of Affective Value and Condition of Learning upon Efficiency of Learning and Galvanic Skin Reflex Deflections

The figures in Tables I and II showed that the affective value of the materials did not affect either the amount and speed of recall or the number and magnitude of galvanic skin reflex deflections elicited during learning. The figures in Tables III, IV, V, VI, VII and VIII, on the other hand, revealed that the attitude assumed during the presentation of the lists of paired associates did influence these measurements. It was found that under the active condition of learning the amount and speed of recall was greater and also the number and maximum magnitude of deflections than under the

TABLE VII

AVERAGE NUMBER OF DEFLECTIONS ELICITED PER LIST

Name	Active Attitude	Passive Attitude	Difference	
			More with Active Attitude	Percent More with Active Attitude
Ca	3.83	1.23	2.60	211
Da	4.00	1.13	2.87	254
Fi	2.54	.38	2.16	567
Fs	2.69	.79	1.90	241
Ha	3.10	.75	2.35	313
He	2.33	.54	1.79	331
Ho	2.73	.73	2.00	274
Hw	3.00	.57	2.43	426
Is	2.96	.57	2.39	419
Ma	2.10	.46	1.64	357
Ob	2.15	.52	1.63	313
Pa	3.98	.57	3.41	598
Si	3.52	1.00	2.52	252
Sm	3.46	.92	2.54	276
Su	3.40	1.08	2.32	215
Ue	2.27	.92	1.35	147
Vo	2.83	.92	1.91	208
Wh	4.00	1.10	2.90	264
Wm	3.19	.94	2.25	240
Wo	3.08	.92	2.16	235
Average	3.06	.80	2.26	283

N—Individual—48.

N—Group—960.

TABLE VIII

AVERAGE MAXIMUM MAGNITUDE PER DEFLECTION ELICITED

(In Milliammeters)

Name	Active Attitude	Passive Attitude	Difference	
			Larger with Active Attitude	Percent Larger with Passive Attitude
Ca	.0296	.0181	.0115	64
Da	.0305	.0194	.0106	55
Fi	.0316	.0200	.0116	58
Fs	.0362	.0150	.0212	141
Ha	.0298	.0256	.0042	16
He	.0325	.0170	.0155	91
Ho	.0349	.0294	.0055	19
Hw	.0319	.0163	.0156	96
Is	.0382	.0207	.0175	85
Mo	.0246	.0136	.0110	81
Ob	.0301	.0208	.0093	45
Pa	.0314	.0278	.0036	13
Si	.0352	.0204	.0148	73
Sm	.0293	.0225	.0068	31
Su	.0285	.0252	.0033	13
Ue	.0302	.0236	.0066	28
Vo	.0328	.0168	.0160	95
Wh	.0285	.0234	.0051	22
Wm	.0196	.0122	.0074	61
Wo	.0198	.0168	.0030	18
Average	.0302	.0203	.0099	49

N—Group—2936. 768.

passive attitude. The question now arises as to whether an analysis of the data for the active and passive conditions separately will disclose that learning efficiency and galvanic skin reflex deflections were influenced by the affective value of the materials. For the answer to this question, we may refer to Tables IX, X, XI, and XII.

TABLE IX

AVERAGE NUMBER OF ITEMS RECALLED PER LIST AND AVERAGE TIME OF RECALL PER ITEM RECALLED UNDER THE ACTIVE CONDITION OF LEARNING

Affective Value	No. per List	Aver. No. Recalled per List	σ (dis.)	Total No. Recalled	Aver. Time in Seconds	σ (dis.)
Pleasant	4	3.352	.809	3218	1.872	1.127
Unpleasant	4	3.332	.818	3199	1.856	1.074
Indifferent	4	3.329	.847	3196	1.752	1.076

TABLE X

AVERAGE NUMBER OF ITEMS RECALLED PER LIST AND AVERAGE TIME OF RECALL PER ITEM RECALLED UNDER THE PASSIVE CONDITION OF LEARNING

Affective Value	No. per List	Aver. No. Recalled per List	σ (dis.)	Total No. Recalled	Aver. Time in Seconds	σ (dis.)
Pleasant	4	2.538	.850	2437	1.873	1.050
Unpleasant	4	2.490	1.076	2391	1.938	1.116
Indifferent	4	2.509	1.089	2409	1.941	1.158

TABLE XI

AVERAGE NUMBER OF DEFLECTIONS ELICITED PER LIST AND AVERAGE MAXIMUM MAGNITUDE PER DEFLECTION ELICITED UNDER THE ACTIVE CONDITION OF LEARNING

Affective Value	Aver. No. Elicited per List	σ (dis.)	Total No. Elicited	Aver. Max. in Milliammeters per Deflection	σ (dis.)
Pleasant	1.086	1.210	1043	.0317	.0364
Unpleasant	.962	1.100	924	.0285	.0352
Indifferent	1.009	1.110	969	.0302	.0389

Tables IX, X, XI and XII show that the attitude assumed during learning did not exert a discriminating influence upon the nature and the degree of the affect of the affective value of the materials upon either learning efficiency or galvanic

TABLE XII

AVERAGE NUMBER OF DEFLECTIONS ELICITED PER LIST AND AVERAGE MAXIMUM MAGNITUDE PER DEFLECTION ELICITED UNDER THE ACTIVE CONDITION OF LEARNING

Affective Value	Aver. No. Elicited per List	σ (dis.)	Total No. Elicited	Aver. Max. in Milliammeters per Deflection	σ (dis.)
Pleasant	.283	.566	272	.0221	.0315
Unpleasant	.230	.524	221	.0196	.0295
Indifferent	.286	.512	275	.0191	.0269

skin reflex deflections. The differences between the results for the three types of materials as they appear in the tables are quite small; a computation of the probable errors showed these differences to be unreliable.

PART IV. RELATION OF EFFICIENCY OF LEARNING AND GALVANIC SKIN REFLEX DEFLECTIONS

In order to ascertain whether there is an actual association between efficiency of learning and galvanic skin reflex deflections, and, at the same time, to rule out the factor that all items that were recalled did not elicit deflections during their presentation, we used the Contingency Method developed by Pearson. The fundamental principle underlying the method is a comparison of the frequency of association actually found in an investigation with the frequency we should expect to find if the variables being studied were completely unrelated, that is, through a chance relationship alone. Table XIII summarizes the values that were actually obtained in this experiment and the values that would have held through chance alone.

TABLE XIII

Values	Average with Deflections Recalled	Average with Deflections not Recalled	Average without Deflections Recalled	Average without Deflections not Recalled
Actually obtained	145	40	698	269
Obtained through chance	136	49	707	260

These figures indicate that the frequency with which recall and galvanic skin reflex deflections were actually related

differed only slightly from the frequency with which they were related through chance. We must conclude, therefore, that the fact that an item elicited a deflection during its presentation did influence the possibility of its being recalled more readily than if it had not elicited a galvanic skin reflex reaction.

Summary

The purpose of this investigation was to study the interrelationships of the following factors: (1) the affective value of the materials used in learning; (2) the volitional attitude assumed during learning; (3) the efficiency of learning; and (4) the galvanic skin reflex deflections elicited upon the presentation of the materials. The general method of approach to this problem was as follows: Subjects were presented with lists of words that contained paired associates of a pleasant, unpleasant, and indifferent character. They were instructed to assume either an active or a passive attitude while learning a list. Each list was presented but once, and recall followed immediately after presentation. The efficiency of learning was measured in terms of the number of items recalled and the speed of recall. Measure of the maximum magnitudes of the galvanic skin reflex deflections elicited during learning were obtained. The findings of this investigation were that: (1) the affective value of the materials did not influence either the efficiency of learning or the galvanic skin reflex deflections elicited during learning; (2) the active condition of learning was more efficient with regard to both the amount and speed of recall and the amount and magnitude of galvanic skin reflex deflections elicited; (3) the affective value of the materials still failed to exert any influence when the data for the active and passive conditions were analyzed separately; and (4) there was no relationship between the galvanic skin reflex deflections and efficiency of learning.

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FACTORS INFLUENCING THE MEMORY VALUE OF WORDS IN A SERIES

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The frequent use of serial memory for words as a testing device in the study of factors influencing the memory process is adequate excuse for the numerous studies which have attempted to analyze out the factors influencing the memory value of a particular word. The experiment here reported was begun in an attempt to correct some of the errors in technique of previous investigations on the relation of affective tone to memory value of words. When a satisfactory method had been devised for the study of this problem, it was found that certain other important data were available. The effect of frequency of usage of a word on its memory value has not, so far as we know, been reported. The effect of the number of letters, while apparently unimportant, may not be so. The effect of position in the series has been studied upon numerous occasions with techniques which frequently have been quite inadequate.

Since the literature on the first and last of the problems mentioned is quite voluminous, a careful survey of the literature would be unprofitable.[1] Certain points on technique, however, deserve special mention.

Affective Tone.—The most important error in studies attempting to investigate the relation of affective tone to memory has been an incorrect criterion of feeling-tone. Two incorrect criteria have frequently been used. One is that of accepting the rating of one individual (usually the experimenter) as typical and studying the memory value of words rated as pleasant and as unpleasant by him. This method

[1] The studies on feeling-tone and memory have been reviewed by Meltzer (3). The primacy-recency problem has been summarized up to 1926 by Robinson and Brown (4).

ignores the wide individual differences in affective rating of words (adequate evidence for which is presented herewith). Another error lies in the use of the galvanometer as an indicator of 'feeling-tone.' Whatever else may be doubtful about the concept of 'affection,' it certainly is a conscious phenomenon and cannot be defined in physiological terms.

A second important error made rather uniformly by experimenters in this field has been the failure to control the position factor. A word has regularly been placed in a certain position in the list and kept in that position, hence the results measure not only the memory value of that word but also the memory value of that position.

To control these variables it is necessary (1) that each subject rate his own words and (2) that each word occupy *every* position in the list an equal number of times.

Primacy-Recency.—The effect of the position in a sequence upon memory value has long been noted. Many investigators, however, have been content to use, *e.g.*, several lists of nonsense syllables and average the correct recalls for each serial position, assuming that 'chance' would rule out any variables other than position which affected the results. The only adequate method of insuring uniform opportunities for each position in the series is that of having each member of the series occur an equal number of times in each position. Foucault (1) used this method in a study of 'inhibitions of fixation.' [2]

It will be seen, then, that if each member of the series occurs equally often in each position, we have separated the memory value of the specific member from the memory value of serial position and can analyze each at our leisure. Such a technique was followed in the present study.

Procedure

The immediate point of departure of this study was the work of Smith (5) on the relation of galvanometric deflection (referred to by him as affective tone) to memory value. He gives a list of 100 words with weighted galvanometric values for each. These have been confirmed in this country by Jones (2).

[2] The fact that Foucault's results bore on this problem was concealed by the title of the paper, and was discovered only after the conclusion of this experiment.

Twenty of the twenty-two words which were highest in average G.D. and ten of the lowest eleven, making a total of thirty words, were selected for this experiment.[3] The number was a matter of convenience in checking Smith's conclusions—the first group contained a number of 'positive' and a number of 'negative' words, and the last group should contain mostly indifferent words.

These thirty words were arranged in a random order and typewritten. The order remained constant throughout, but the first member of the series changed with each typing: thus the first sheet was 1–2–3 . . . 30, the second, 2–3–4 . . . 30–1, the third, 3–4–5– . . . 30–1–2, and so on. In this way, when thirty copies of the list had been made, each word had occupied every position in the list.

Experimentation was done in groups. The typewritten sheets were passed out and each subject rated the words as pleasant, unpleasant or indifferent. He then turned the sheet face down and waited for instructions. When all had finished, they were told that they were to memorize the list, using the whole method, being allowed 150 seconds for this. When time was called, each took a sheet of manila paper (previously furnished), signed his name and wrote from memory as many words as possible. Order was not asked for in the recall. A total of 200 college students served as subjects.

Results

The results may be presented in two ways: by subjects, as is customary, and by words. In Table I are summarized the

TABLE I

Summary of Results Calculated by Individual Subjects

	N	M	S.D.
No. words rated: P	200	13.26	2.74
U	200	9.09	1.92
I	200	7.65	3.05
No. words recalled: P	200	9.95	2.90
U	200	5.97	2.23
I	197	5.55	2.50
Percent words recalled: P	200	75.20	15.43
U	200	65.30	18.88
I	197	73.23	19.63

average number of words rated in each feeling category by each subject, the average number in each feeling category which were recalled, and the percentage recalled. For example, an average subject would rate thirteen words pleasant, recall ten of these thirteen, and thus obtain a percentage score of about 76 for pleasant words. (By computing percentages separately for each subject we were enabled to calculate the S.D. and reliability of a difference.)

[3] 'State' and 'war' were omitted from the first twenty-two because we desired to tap the personal feelings of our subjects. 'Bury' was omitted from the last eleven because it was misunderstood (oral presentation by Smith) as 'berry.'

TABLE II

MEAN DIFFERENCES FOR ITEMS IN TABLE I

	D	S.D.$_D$	C.R.
No. words rated: P–U	4.17	0.235	17.74
P–I	5.61	0.288	19.47
U–I	1.44	0.253	5.69
No. words recalled: P–U	3.98	0.258	15.44
P–I	4.40	0.271	16.25
U–I	0.42	0.242	1.73
Percent words recalled: P–U	9.90	2.970	3.33
P–I	1.97	3.147	0.62
I–U	7.93	3.740	2.12

In Table II are presented the differences between the different means shown in Table I. Those which are really significant are the percentage differences. There is an average difference of 9.9 percent between pleasant and unpleasant. This difference is 3.33 times its standard error and therefore meets the conventional requirements for a statistically 'true' difference. The indifferent words are also superior to the unpleasant, but the difference here is only 2.12 times the standard error. Since the present figures are based on 200 cases, we doubt that any difference of practical value exists between the indifferent and unpleasant words.

TABLE III

FREQUENCY DISTRIBUTIONS FOR THE PERCENTAGES RECALLED AS SUMMARIZED IN TABLE I

	P	U	I
1– 10	0	0	0
11– 20	1	2	2
21– 30	1	6	4
31– 40	3	12	9
41– 50	8	36	17
51– 60	19	28	20
61– 70	42	36	26
71– 80	49	36	40
81– 90	43	28	40
91–100	34	16	39

Table III lends point to another aspect of the problem. It has been customary to speak in terms of average memory value and mean differences. In Table III are shown fre-

quency distributions for the percentage recall of pleasant, unpleasant and indifferent words, and it is quite obvious that there is 100 percent overlapping of the distributions. We consider this point quite important as a modification of the frequently made assertion that unpleasant ideas will be quickly forgotten. In any particular individual, the pleasant idea may or may not be more effectively recalled, and only in a large group under the carefully controlled conditions of an experiment is this difference likely to be significant.

TABLE IV

SUMMARY OF RESULTS CALCULATED BY INDIVIDUAL WORDS

(The galvanometric deflections given are from Smith)

	G.D.	Mem. V.	P	Percent P	U	Percent U	I	Percent I
Kiss	72.8	94	145	95	9	88	46	91
Love	59.5	89	177	88	2	100	21	90
Marry	58.5	82	107	78	23	91	70	85
Divorce	50.8	86	4	75	147	87	49	85
Name	48.7	57	64	54	4	100	132	57
Woman	40.3	88	121	89	5	100	74	86
Wound	38.0	53	2	50	186	52	12	75
Dance	37.4	71	128	77	25	56	47	63
Afraid	36.8	56	3	100	176	53	21	66
Proud	36.7	55	36	66	114	51	50	56
Habit	36.6	57	43	60	51	60	106	54
Money	35.6	64	171	63	10	60	19	68
Fight	35.0	76	30	86	146	76	24	66
Child	35.0	87	149	88	3	100	48	83
Despise	34.7	71	3	66	183	71	14	78
Family	33.6	78	171	77	7	71	22	81
Happy	33.4	62	198	62	0	—	2	50
Pray	33.1	68	130	67	11	81	59	66
Worry	33.0	56	0	—	190	55	10	70
Insult	32.5	66	1	100	186	65	13	76
Work	18.3	81	103	80	58	84	39	76
Carrot	18.2	81	100	82	25	92	75	77
Hunger	17.9	56	23	47	165	58	12	41
White	17.8	81	110	80	9	88	81	80
Glass	17.6	75	44	79	23	78	133	72
Give	16.7	68	143	67	7	71	50	72
Flower	16.1	64	189	65	0	—	11	54
Pond	15.5	80	73	84	28	85	99	74
Pencil	15.4	65	21	57	10	40	169	68
Swim	14.2	69	163	70	15	73	22	54

The results were also tabulated by individual words, and these are shown in Table IV. The words are shown, not in the order in which they were used in this experiment, but arranged in accordance with the size of the galvanometric

deflection reported by Smith. This is indicated in the first column. The second gives the memory value of the word in the present experiment (*e.g.*, 'kiss' was recalled by 188 of 200 subjects, hence the memory value in percentages is 94). P, U and I indicate the number of times the word was given a certain rating; percent P, percent U and percent I indicate the relative frequency of recall when the word occupied a given feeling category.

A brief examination of this table is surprising. We find that only ten of the thirty words are remembered more frequently when rated pleasant than when rated unpleasant. On the other hand, sixteen words have a higher score when unpleasant than when pleasant. The average of the percent P column is 74.2, while that of the percent U column is 74.5. Clearly, when we consider individual words, there are no significant differences in the memory value in accordance with the subjectively reported feeling-tone.

A P–U potency index was constructed by the simple device of subtracting the number of unpleasant ratings from the number of pleasant ratings. This enables us to rank these words according to a rough scale of pleasantness-unpleasantness. A rank-correlation of this measure with memory value was found to be + .375, indicating that there is a respectable degree of relationship between the number of people rating a word pleasant and the number recalling it.

A conclusion on the general significance of subjective feeling-tone as related to memory is therefore rather hard to reach. A conservative statement of the case would be that, given a large number of people and a large number of words, there will be a significant relationship between pleasantness and memory value.

The galvanometric deflection occurring in response to a given word does not seem to be related to the memory value of the word. There is a slight tendency for words having a high G.D. to be better remembered, as indicated by a rank correlation of + .193, but this relationship is so small as to be negligible. The curvilinear relationship alleged by Smith is not borne out by the present data, as anyone can see who

cares to plot the obtained memory values against the G.D. values.

The frequency of use of the thirty words used in this experiment was estimated by the frequency values given in Thorndike's Word Book. While this is of course quite inaccurate, it is the best available. The correlation of memory value with frequency (rank-difference) is + .161. A comparison of the fifteen most frequent words with the fifteen least frequent confirms the indicated slight relationship: the average memory value of the first fifteen is 73.0, and of the second fifteen, 69.3.

These results with respect to the frequency of usage of the word justify only the general conclusion that, in material which has been strongly overlearned, frequency of use is probably unimportant.

The size of the word is apparently of some importance in determining memory value, although the range here is very narrow (four, five, six and seven-letter words). A rank correlation of − .224 between memory value and number of letters indicates that the shorter words are best remembered. It is probable that a wider range of words would confirm this conclusion more strikingly.

The above factors operate, as we have said, independent of the position of the word in the series, since we have carefully arranged our situation so that each word held each position an equal number of times. We may now consider the data on the memory value of a position irrespective of the word occupying it.

Table V shows the arrangement of the words (positions) and the number of times the word in that position was recalled, of 200 presentations. The words were arranged in two columns of fifteen each, so that we can compare the right and left-hand columns also.

It will be noted that the superiority of primacy is verified by this table. In the first column (left), there is a smooth descent from the first to the eighth member, then an intervening mode and a slight rise at the end, if the results are plotted graphically. In the right-hand column the curve is less smooth, as the third member is out of line. The curves

TABLE V

MEMORY VALUES FOR POSITIONS IN A SERIES, INDEPENDENT OF THE WORD OCCUPYING THE POSITION

Position	Times Recalled in 200	Position	Times Recalled in 200
1	191	16	157
2	182	17	145
3	171	18	150
4	166	19	138
5	156	20	130
6	148	21	133
7	133	22	130
8	130	23	121
9	147	24	127
10	149	25	128
11	153	26	113
12	141	27	126
13	136	28	132
14	145	29	129
15	148	30	141
Average of left-hand column	153.06	Average of right-hand column	133.33

from the two columns run fairly well parallel to each other, the rank correlation of the two being + .638. This consistency seems to indicate that there is a definite tendency for similar positions to have similar memory values.

The curve for the right-hand column is at all points lower than that for the left-hand column, confirming the general belief that the left-hand side of a page is the more desirable space for advertising. The modes in the curves may very possibly be due to fluctuations in attention, the subject tending to concentrate for a few seconds and then wander for a similar period.

DISCUSSION

The above results interpret themselves. While we are somewhat prejudiced in favor of the view that pleasant material is retained longer than unpleasant, we do not wish to strain the data to give color to that point of view. A fair degree of relationship between feeling-tone and memory value is indicated. This relationship is more marked when the data are analyzed by subjects than by specific words, and it seems probable that the general conclusion should be qualified by the statement that these results are valid only with a large number of subjects and under experimental conditions.

The results on frequency of usage and size of word indicate that these factors are not of great importance in determining the value of a word in an immediate memory test. We suspect that these factors would be of greater significance in a delayed recall or with material not so completely overlearned as this.

The traditional importance of primacy and recency, and the superiority of the former over the latter, are confirmed by the material on position value. The left-hand column is distinctly better than the right hand, the difference amounting to 20 recalls in 200, or 10 percent. Fluctuations of attention within the column are probably causes of the irregularity noted in the curves representing the relation of position to memory value.

Summary

1. A significant difference favoring the retention of pleasant over unpleasant words in immediate memory is found.

2. This difference is not observable when the data are examined for each word separately, but only when the results are lumped.

3. The frequency of usage of a word does not seem important in determining its memory value.

4. The galvanometric deflection caused by oral presentation of a word does not seem related to memory value.

5. The number of letters in the word is slightly related to retention, shorter words being better recalled.

6. The traditional values of primacy and recency in determining memory value are confirmed, and the superiority of primacy over recency is clearly indicated.

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THE ALLEGED WARMING-UP EFFECT IN MEMORIZATION [1]

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Robinson and Heron [2] found a warming-up effect in experiments on tapping and saying the alphabet backwards. Heron [3] later reported what he called a warming-up effect in the memorization of nonsense syllables. He referred to Amberg's [4] report of warming-up effects when numbers were used and Thorndike's skepticism regarding its validity. Amberg gives no reliability coefficients.

Heron used the *paired association* method of learning nonsense syllables. He gave a two-second exposure, with about two minutes between lists. He concludes that the differences found in terms of trials and errors was not due to practice because he had unreliable differences when he took averages for lists in the order learned regardless of the condition, *i.e.* regardless of the session in which it was learned. Heron writes, "Nor are these differences due to the transfer of specific habit-units from the first list to the second in each condition, for if there were such transfer it should have the same effect upon the learning of subsequent lists as practice factors, unless one is willing to assume that these habit-units were forgotten during the week interval between each condition and thus could not be used to facilitate the learning of the subsequent lists." [5] To the present writer, it seems

[1] This study was carried out in the Psychological laboratory of Yale University. The writer is indebted to Professor Clark L. Hull for use of apparatus and guidance in the performance of the experiments upon which this article is based, and to Professor Edward S. Robinson under whose personal direction they were performed.

[2] Robinson, E. S. and Heron, W. T., The warming-up effect, *J. Exp. Psychol.*, 1924, 7, 81–97.

[3] Heron, W. T., The warming-up effect in learning nonsense syllables, *J. Genet. Psychol.*, 1928, 35, 219–228.

[4] Amberg, E., Ueber den Einfluss von Arbeitspausen auf die geistige Leistungfähigheit, *Psychol. Arbeit.*, 1892, 1, 300–377.

[5] *Ibid.*, pg. 226.

altogether possible that the habit-units were forgotten during the week interval especially since only one day of practice was given and only four sessions in all were given separated by a whole week.

Materials, Apparatus, and Subjects

In the work reported here, sixteen Yale students learned lists of ten three-place numbers exposed for two seconds on a memory drum. Most of the time, the drum used was one designed by McGeogh, but during a little preliminary experimentation Hull's memory drum was used.

Procedure

The results reported here were noticed incidentally while performing some experiments on hypnosis and waking suggestion using the retroactive inhibition set-up.[6] The subjects learned at least two lists of ten three-place numbers per day, five to six days per week for from 16 to 84 days (except for three subjects who were discontinued before the practice period of the experiment was completed and so are included only in the results considering three sessions). The *anticipation* method was used and the numbers learned to one correct repetition in the original order instead of to two perfect repetitions, each in a different order as used by Heron. The procedure and material is quite different in the two cases, but both involved learning two lists of relatively meaningless material. Heron describes his lists as being given in 'immediate succession' but stated there was about a two minute interval between them. The bulk of the work reported here on eleven subjects was given with only about a forty second interval between lists. In order to make certain that the difference in the results was not due to the length of this relatively short interval between lists, the writer figured the differences for two subjects previously run for 76 sessions each with a two minute interval between lists.

[6] Mitchell, M. B., Retroactive inhibition and waking suggestion. *J. Abn. & Soc. Psychol.*

Mitchell, M. B., Retroactive inhibition and hypnosis. *J. Gen. Psychol.*

Results

The results for eleven subjects after a practice period of from 8 to 11 days furnishes the main part of this report. Six of them were run for 24 days, and five of them for 8 days. Table I shows almost an identical number of trials needed to

TABLE I

Trials	List 1	List 2
Σ	2061	2043
Mean	11.2	11.1
P.E.	3.89	3.33

Trials to Learn Lists 1 and 2: 11 Subjects, 184 Sessions.

learn both lists. The difference in the mean is only .1. The Probable Error of this difference is 3.61, giving an R, reliability of a difference coefficient,[7] of .03 which is no difference at all. The results are similar in terms of errors (Table II). Although here the number of errors is greater for the second list learned than for the first, there is still no reliable difference; R is only equal to .05.

TABLE II

Errors	List 1	List 2
Σ	11,288	11,865
Mean	61.3	64.5
P.E.	21.13	17.90

Errors to Learn Lists 1 and 2: 11 Subjects, 184 Sessions.

It was conceivable that these same subjects might show reliable differences early in the practice period (the point where Heron's work was done), although Heron claimed that his results were not due to practice effects. Since he gave only three sessions besides one practice period to each subject, the number of trials to learn the first list on the second, third, and fourth days during the practice period for these 11 subjects and 3 others were compared with the number of trials to learn the second list on these days. The subjects had already had the equivalent of Heron's practice period; they

[7] Found by dividing the difference of the means by the P.E. of the difference.

had learned two lists. The sessions were held on successive days rather than successive weeks. The results are shown in Table III. R is somewhat larger here than it was after more practice but is still in no way comparable to those obtained by Heron; it was only .37.

TABLE III

Trials	List 1	List 2
Σ	1194	1090
Mean	28.4	26.0
P.E.	8.59	10.18

Trials to Learn Lists on 3 Successive Days after One Day's Practice: 14 Subjects.

The preceding results were with a 40 second interval between lists. The two subjects used in a previous experiment with a two-minute interval (the same as Heron reports having used), showed no reliable differences in trials or errors. For 76 sessions the mean number of errors for list 1 was 41.25 and for list 2 was 46.42. The mean number of trials for list 1 was 8.08 and for list 2 was 8.55 with an R of .32.

While there is no real quantitative difference in the number of trials or errors in learning the two successive lists of numbers, there might be qualitative differences. It was observed during the experiment that there were more zero-digit responses [8] for list 2 than for list 1.[9] For the 11 subjects used in this experiment the total number of zero-digit responses for list 1 was 8,099 or 71.8 percent of the 11,278 errors, and for list 2 was 8,970 or 75.6 percent of the 11,864 errors. In terms of the percent of the total errors made in learning the lists, there are nearly 4 percent more zero-digit responses for the learning of the second list than for the first.[10] Since the greatest differences were observed to be early in the learning of the lists, the number of correct responses in the second, third, and fourth trials for the learning of each list were compared. The mean number of correct responses for list 1 was 8.6 as

[8] A zero-digit response means that no verbal response was given; the subject gave no digits during the exposure of the preceding number.

[9] Mitchell, M. B., Errors in the memorization of numbers. *Am. J. Psychol.*

[10] This difference is about twice as great as is found for any of the other types of errors. See *ibid.*

opposed to only 5.6 for list 2. The probable errors, however, are very large, 2.83 and 1.95 respectively, so that the difference can scarcely be considered reliable since R is only 1.09. Nevertheless, the results are much more consistent than reliable when the individual subjects are compared. Ten of the eleven subjects show a larger total number of correct responses for these three trials in list 1 than for these three trials in list 2 (Table IV). The one subject, Eve, who showed

TABLE IV

Subject	List 1	List 2
Hic	35	30
Wes	22	14
Orc	73	58
Eve	15	19
Qui	58	49
Cob	182	155
Ber	279	169
Rab	226	152
Awd	184	114
Sam	276	158
Bri	237	118
Σ	1587	1036
Mean	8.6	5.6

No. of Correct Responses in Trials 2, 3, and 4 by Subjects: 8 Sessions for First 5 Subjects, and 24 Sessions for Last 6 Subjects.

fewer correct responses for list 1, was one of the five subjects only run for eight sessions. He differed from all the other subjects in several respects; the only one of importance here is that he showed fewer correct responses for both lists for these trials than any of the others. Hence the total number of correct responses was rather too low to be of much significance; he had only 15 correct responses for list 1 and 19 for list 2.

Conclusions

There were no reliable quantitative differences found between the number of trials or errors in learning consecutively two lists of ten three-place numbers when the subjects are given daily trials. There is, however, some evidence for a qualitative difference which is more consistent than reliable.

There is some tendency to give more zero-digit responses in the learning of the second list. This failure to make attempts may be responsible for the smaller number of correct responses made in the second, third, and fourth trials of the learning of the second list as compared with the learning of the first list. It might be considered as a form of associative inhibition.

(Manuscript received October 12, 1931)

STUDIES IN CROSS EDUCATION

I. Mirror Tracing the Star-shaped Maze

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It is well known that training given to one muscle group will carry over to the symmetrical muscle group on the opposite side of the body. Beginning with Weber, a long line of investigators have noted this phenomenon, usually called cross-education. Outstanding experiments in the field have been performed by Scripture, Smith, and Brown on steadiness of hand, Woodworth on ability to hit a target, and Starch and Ewert on mirror drawing. An excellent review of previous work is given by Bray.[1]

A research conducted thirty years ago by Davis and a more recent investigation by Bray seem to give the problem a wider setting. Davis found that improvement in rate of tapping by practice with the right toe resulted in a large gain in succeeding trials with the left toe, left hand, and right hand. Bray discovered that training in hitting at a target seen in a mirror, when given to the right hand, resulted in increased efficiency of the right foot at the same performance. Gain with the right foot was also carried over to the right hand, though to a less degree. When we consider these results the problem assumes a more general form. Does training given to one muscle group result in improved functioning of all the muscles of the body? If so, what are the conditions which favor or work against such muscular transfer? In this form, it is conceivable that an analysis of the factors in the problem might throw light on the learning of action patterns and their connection with perception.

The special phase of the problem attacked in the present investigation was a modification of the familiar task of

[1] C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 1928, 11, 443–467. The article includes a complete bibliography of previous work.

tracing a six pointed star seen in a mirror. In an orientation experiment with the writer as subject, it was found that 100 trials practice in mirror tracing a six pointed star with the left foot [2] resulted in a marked gain at the same task with the right hand. The superiority of hand over foot in the use of a pen or pencil, however, made comparison between the two difficult. It seemed desirable to devise a type of apparatus which minimized the role of motor skill and emphasized the direction component, since it seems that the novel factor in mirror tracing is a change in the relationship between vision and direction of muscular movement.

The apparatus finally constructed was a maze in the shape of a six pointed star, with blind alleys at all points where the direction of movement changes. The maze, as seen in Figs. 1 and 2, was built in three layers. The lower layer is a hexagonal three-quarter inch wooden base seven inches on a side, and thus 42 inches in circumference. The second layer consisted of blocks mounted on the base, and was likewise three-quarters of an inch thick. The top layer was of three-ply wood one-quarter inch in thickness. The number and shape of the pieces making up the top may best be seen in Fig. 2. Inside the one-quarter inch moulding, and each in the centre of one of the six sides of the maze, are six triangular pieces two inches on a side. Between each triangular piece is a wooden hexagon two inches on each of the four shorter sides and two and one-half inches on the two remaining sides. The central block is a regular hexagon two inches on a side. All parts are of wood except the triangular pieces of heavy tin, one and one-half inch on a side, which lie one-half inch from the central block. The star-shaped 'correct' path, one-half inch wide and with two blind alleys at each of the twelve turns, is clearly outlined in Fig. 2. Each lap of the correct path is two and one-quarter inches long, and the blind alleys leading toward the outer edge are two inches long. The twelve inner blind alleys could only be made three-quarters of an inch in depth, or they would meet and form an alternate hexagonal path shorter than the star-shaped path.

A cross-section of the maze path in Fig. 3 shows the construction of the intermediate layer. The blocks making up this layer are similar in shape to those making up the upper layer, but the former are three-quarters of an inch thick and one-quarter of an inch less in length and breadth on all sides bordering on the maze path. The lower path through the maze is thus three-quarters of an inch deep and one inch wide. Since the wooden tip of the metal stylus is three-quarters of an inch in diameter and one-quarter of an inch thick, it is retained and yet can move freely in the lower path. The tracing apparatus is shown in Figs. 1, 2, and 3. The stylus is an one-eighth inch brass rod bent at right angles one and one-half inches from one end, which is fixed in the wooden tip mentioned above. The opposite end of one such rod was driven into a round wooden handle four inches long and one-half inch in diameter, for use in tracing with either hand. Other rods were driven into wooden sandals cut out on a band saw to the shape of the right and left foot. The sandals are held securely to the feet by leather straps and kept from slipping forward by a copper cleat rounded

[2] The pencil was fastened to a block of wood nailed on the sole of a shoe.

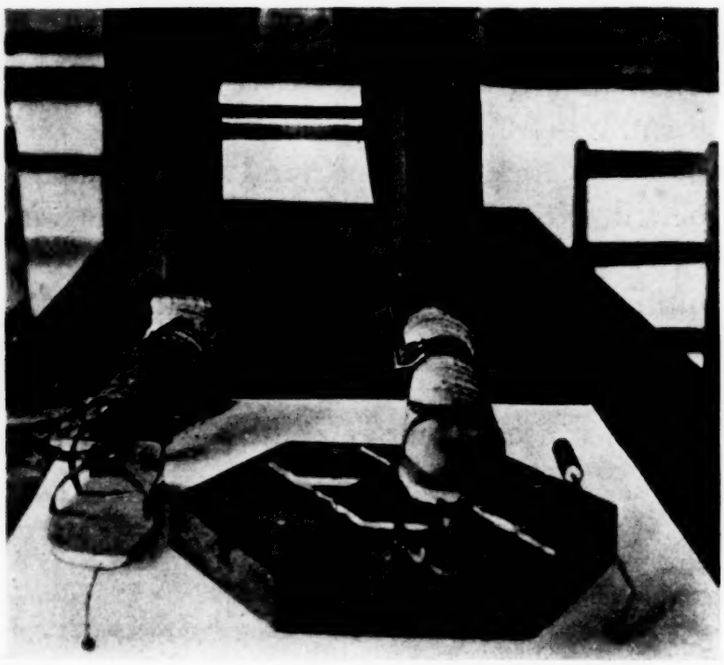

FIG. 1. Star-shaped maze, front view.

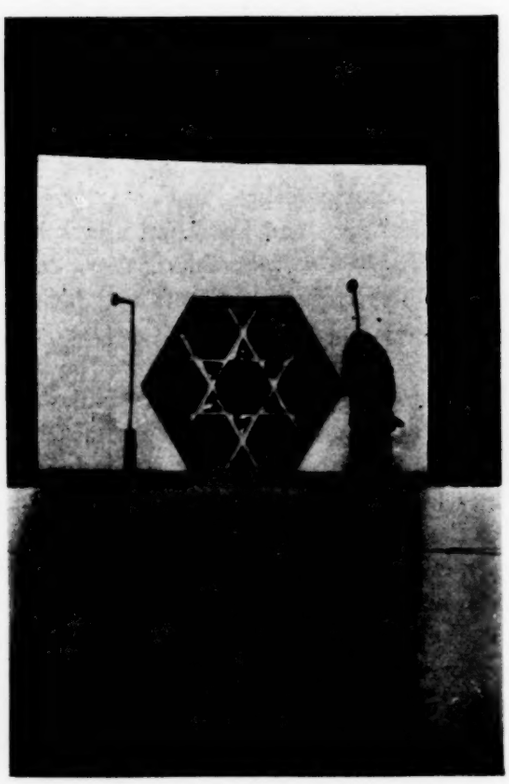

FIG. 2. Star-shaped maze, top view.

to the shape of the foot and nailed on the back of each sandal. When tracing with the foot the subject sat on a chair and the maze was placed on the floor. The subject stood while tracing with either hand, and the maze was placed on a table at a comfortable height. The former arrangement is shown in Fig. 1. The mirror, which was eleven by seventeen inches, and an adjustable screen of three-ply wood for concealing the maze from the subject, are not shown in the figure. A wooden platform two

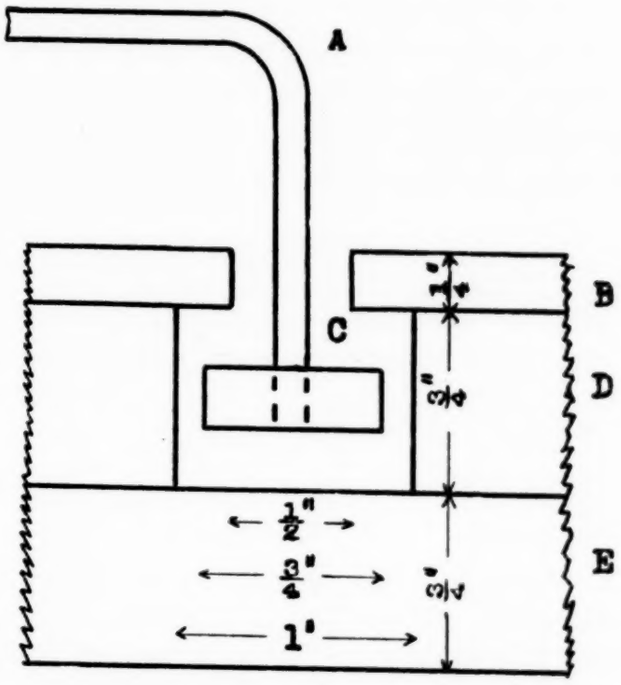

FIG. 3. Cross-section of path in star-shaped maze. *A*, metal stylus; *B*, upper layer; *C*, path of stylus; *D*, intermediate layer, *E*, base.

inches high, which raised the subject's chair slightly and thus made tracing easier, and also held the maze firmly in position, are likewise omitted.

The starting point for tracing was the right of the two blind alleys farthest from the subject, and the finishing point the left blind alley in the same region. In Fig. 1 the subject is at the end of the path. At the beginning of each trial the subject's foot or hand was guided to the starting point. On the first trial he was shown the correct path and instructed to make as good time as possible. A ready signal was given before the actual starting stimulus. Timing was done with a stopwatch, and only the time for each trial was recorded. Errors were not considered, for two reasons. Not only did the subjects often move so quickly that it was extremely difficult to count the errors, but there is reason to believe that time is the more accurate measure. Many subjects proceed cautiously, hesitate at the turns, and find the right direction after incipient movements in the direction of the blind alleys. These movements, though true errors, are too slight to be recorded by any feasible method. Yet both cautious and impulsive subjects are penalized in the time record, the former by their slowness of movement, the latter by the time needed to correct their errors.

Thirty male students of Acadia University and Horton Academy served as subjects. These were divided into three

groups, A, B, and C, of ten subjects each. Trials were distributed between morning and afternoon about equally for the three groups; but each subject, as far as possible, was tested at the same time each day.

On each of two days before beginning mirror tracing, and on one day after the mirror tracing was finished, the subjects were given seven trials with each hand and foot with direct view of the maze. Of the seven, two were orientation trials and were not timed. The averages of the remaining five trials are given for the three groups in Table I. Each value in

TABLE I

TRACING THE SIX-POINTED STAR WITHOUT THE MIRROR. EACH VALUE IS THE AVERAGE OF 50 TRIALS, 5 FOR EACH OF 10 SUBJECTS

Group	Relation to Mirror Tracing	Time in Seconds			
		R.H.	L.H.	R.F.	L.F.
A	Before first day	4.3	4.7	8.1	7.5
	Before second day	4.0	4.3	7.0	7.0 ←
	After *	3.4	3.7	5.8	6.0
B	Before first day	4.4	4.6	7.4	7.6
	Before second day	4.0	4.0	6.3	6.4 →
	After	3.6	3.8	5.6	5.7
C	Before first day	4.1	4.2	6.6	6.7
	Before second day	4.0	3.9	6.0	5.9 →
	After	3.4	3.7	5.0	5.5

* Eight subjects. Two subjects in Group A did not come back for the final tests.

the table is the average of fifty trials, five for each of ten subjects. The arrows on the right indicate the order of trials. With Group A the order was L.F., R.F., L.H., R.H. For Groups B and C this order was reversed R.H., L.H., R.F., L.F. Considering the results for the second day, after the subjects have become used to the maze, we see that there is little difference in speed between left and right, but that the feet take about 60 percent longer than the hands. All the subjects in Group A, and all except one in Groups B and C, were right handed. The exclusion of the records of the two left handed subjects, however, makes no significant difference in the group averages.

From comparison of Group A with Groups B and C we notice that the averages for left and right foot are slightly higher in the first named group. This difference is probably due to the order of trials. There seems to be a slight transfer of learning in the direction L.F., R.F., L.H., R.H. in Group A, and in the reverse direction with the other groups. However, the data do not warrant anything but a tentative conclusion. Further research is also needed to decide whether the drop after practice in mirror tracing is due to the latter or is only an after-effect of the first two days' practice without the mirror.

The mirror tracing, which began the day following the second series of orientation trials with direct view of the maze, comprised ten trials a day for each of ten consecutive days, Sundays excepted. Several subjects in all three groups also missed an occasional day. Group A was practiced with the right hand only, Groups B and C with the left foot. The raw data of 3000 readings obviously cannot be reproduced here, but in order to give some idea of the individual variability, the results for the first and last ten trials for all subjects in Group B are given in Table II. The averages for Groups A, B, and C are shown graphically in Fig. 4. In this graph the first ten points on each curve represent the ten trials for the first day, while other points are averages of the ten daily trials for the separate groups, and are plotted at the mid-point of each interval. Thus trials 11 to 20, taken on the second day, are plotted at 15, and those taken on the third day at 25. The first ten points on each curve are thus averages of ten, while all later points are averages of one hundred readings. The first ten points are plotted singly to facilitate comparison with the three to ten transfer trials. However, after the first day the daily average gives better comparative data, since the lapse of twenty-four hours causes a rise in the curve at the beginning of each day's practice.

The three practice curves in Fig. 4 are much alike in shape. All show an initial sharp drop with negative acceleration. The close resemblance between the curves for Groups B and C is striking, and would seem to indicate that ten subjects is a

TABLE II

First and Last Ten Trials Mirror Tracing the Star-shaped Maze with the Left Foot. Group B

Trials	Time in Seconds										
	Ba	Bb	Bc	Bd	Be	Bf	Bg	Bh	Bi	Bj	Average
1	43.4	28.0	60.0	37.0	48.4	43.0	22.4	15.0	35.0	53.0	38.5
2	30.8	33.0	38.4	31.0	26.4	29.0	14.6	14.2	28.6	40.0	28.6
3	24.0	40.6	27.4	30.0	18.0	17.4	14.0	13.0	35.2	29.0	24.9
4	19.4	36.6	20.8	29.6	21.4	25.0	10.4	14.2	22.4	26.6	22.6
5	19.8	30.6	17.6	26.8	17.0	25.0	13.6	10.0	25.0	32.6	21.8
6	17.8	28.6	16.2	24.2	11.6	18.6	14.0	9.0	22.0	21.6	18.4
7	17.0	20.6	16.8	23.6	10.2	15.6	12.8	8.0	17.0	17.6	15.9
8	13.4	21.2	20.6	21.2	10.2	19.8	12.0	10.0	13.0	26.2	16.8
9	20.0	19.6	16.2	22.6	10.2	14.2	12.2	8.2	16.2	19.0	15.8
10	12.6	16.4	13.8	20.6	11.2	19.2	7.2	13.8	19.8	24.6	15.9
.											
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.											
91	6.0	9.6	8.2	9.6	5.2	7.8	5.0	6.8	7.6	10.0	7.6
92	6.4	5.6	7.4	7.2	5.8	7.0	6.0	6.4	9.0	8.0	6.9
93	7.2	5.0	7.4	6.8	5.2	6.2	4.8	6.2	8.2	7.8	6.5
94	6.0	6.4	7.8	8.0	4.0	6.4	3.6	5.8	5.6	7.8	6.1
95	7.0	8.0	6.8	6.2	3.6	8.0	8.4	5.4	6.8	7.8	6.8
96	6.2	4.8	6.8	5.4	5.4	4.8	5.6	8.0	8.0	5.8	6.1
97	6.8	5.2	5.6	7.6	6.0	7.0	6.0	6.2	4.8	6.8	6.2
98	5.6	5.2	7.6	5.6	4.6	7.0	4.4	4.6	6.8	8.0	5.9
99	5.8	5.2	8.2	6.4	6.2	4.4	6.6	6.4	5.6	8.8	6.4
100	4.8	6.4	7.8	5.2	5.2	8.2	5.0	6.0	6.2	7.8	6.3

sufficient number to eliminate individual variability and thus secure approximately equal groups. The high points at trials 5 and 8 in curve *C* are wholly due to one subject who had a time record on these trials higher than on his first trial. The medians for trials 5 and 8 are both 15.9 seconds, values which lie close to the regular sweep of the smoothened curve. The lower final speed in Group A is not surprising in view of the hand-foot differences in the preliminary tracing without the mirror, as shown in Table I. The extremely small difference between the right hand and left foot in the first trial, however, was quite unexpected. The averages for the first trials in mirror tracing are: right hand, Group A, 35.9 seconds; left foot, Group B, 38.5 seconds; left foot, Group C, 37.2 seconds. The difference between hand and foot in the first trial is less than 4 percent, as compared with 30 percent difference between A and the average of B and C in the final

ten trials. Evidently we cannot say that the superior skill of the right hand gives it any initial advantage over the left foot in mirror tracing.

Immediately after the one-hundredth trial on the final day of the practice series, the subjects were tested to discover whether training with left foot or right hand had carried

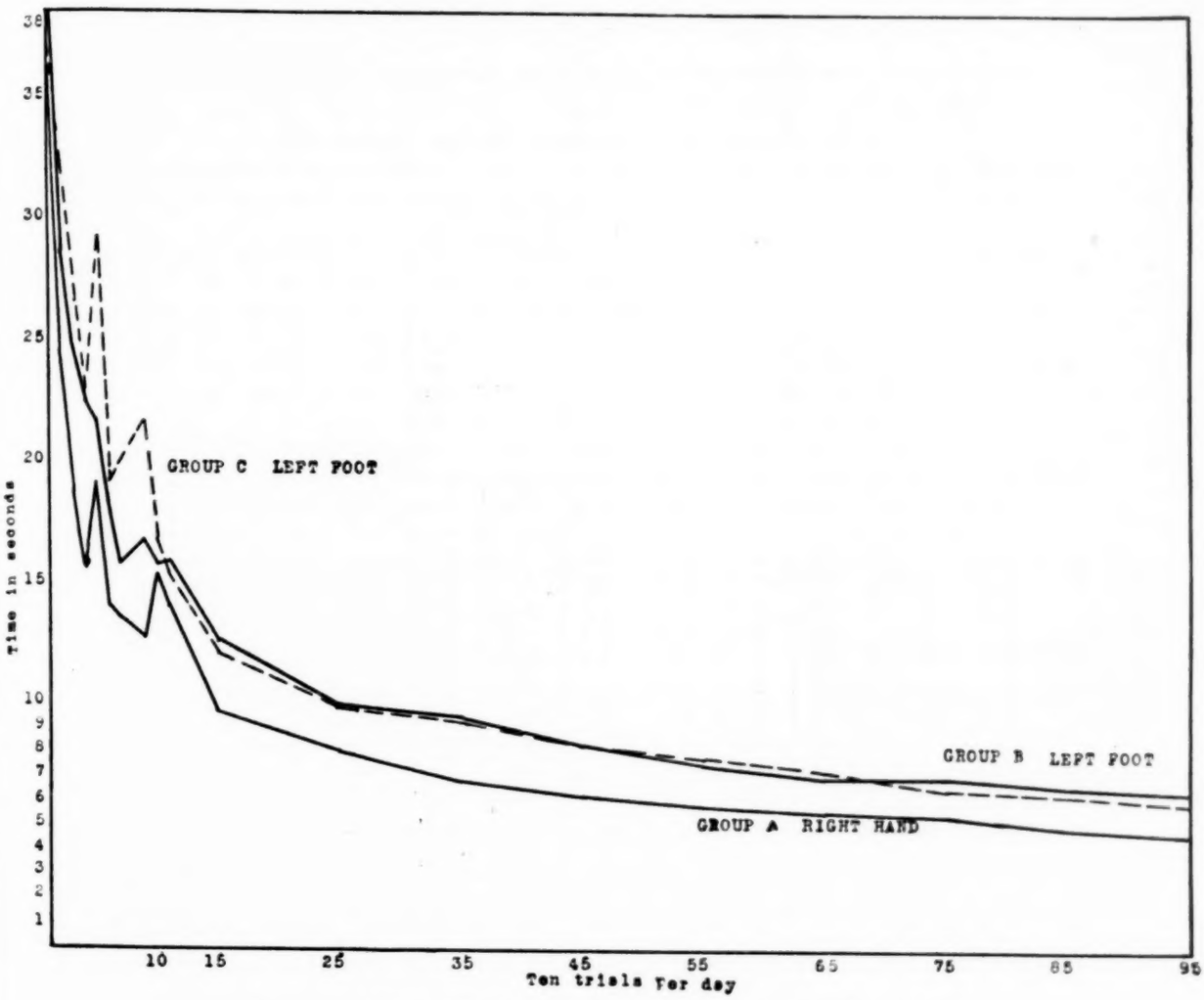

FIG. 4. Mirror tracing the star-shaped maze, practice series.

over to other muscle groups. For Group A, who had been practiced with the right hand, the transfer trials were in the order: left foot (3 trials), right foot (3 trials), left hand (10 trials). The transfer order for Group B was right hand (3), left hand (3), right foot (10). For reasons explained later, although both groups had the initial practice with the left foot, the order of transfer trials for Group C was the reverse of

Group B. The individual readings for Group B are given in Table III and the averages for all three groups in Table IV. In the latter table the column on the left gives the time for the first three of the practice trials. The other three columns show the transfer trials. The great gain in initial speed by all three muscle groups after right hand or left foot practice is

TABLE III

MIRROR TRACING THE STAR-SHAPED MAZE WITH THE RIGHT HAND, LEFT HAND AND RIGHT FOOT AFTER 100 TRIALS WITH THE LEFT FOOT. TRIALS WERE TAKEN IN DESCENDING ORDER. GROUP B

Trials	Time in Seconds										
	Ba	Bb	Bc	Bd	Be	Bf	Bg	Bh	Bi	Bj	Average
1. . . .	13.8	9.6	14.6	14.4	13.2	9.6	10.6	8.6	24.2	7.8	12.6
R.H. 2. . . .	9.0	10.4	11.4	10.0	9.0	8.8	8.2	11.0	12.8	6.8	9.7
3. . . .	8.4	11.0	9.6	6.0	7.6	8.0	4.8	7.0	9.0	6.4	7.8
1. . . .	10.0	16.2	10.4	7.0	6.8	10.6	5.4	6.2	14.8	6.6	9.4
L.H. 2. . . .	5.8	6.4	9.6	8.6	6.4	7.6	9.2	7.0	11.4	7.2	7.9
3. . . .	6.4	5.8	10.0	8.0	6.4	8.4	5.2	5.4	11.8	7.0	7.4
1. . . .	16.2	7.8	12.2	9.2	6.0	9.0	8.0	10.4	15.8	8.0	10.3
2. . . .	14.6	8.8	11.2	10.2	7.2	8.2	6.0	6.0	10.8	7.2	9.0
3. . . .	12.0	6.0	10.2	9.8	7.4	6.4	6.2	6.8	10.8	13.0	8.9
4. . . .	10.6	8.8	11.8	9.6	5.2	7.6	5.4	5.6	7.2	6.0	7.8
R.F. 5. . . .	10.4	6.0	13.2	8.2	5.0	9.2	5.2	8.0	9.2	4.4	7.9
6. . . .	8.8	8.0	9.4	7.0	4.8	7.6	7.2	6.0	10.6	5.6	7.5
7. . . .	9.8	7.4	8.0	9.2	7.6	10.0	5.8	7.8	7.2	5.0	7.8
8. . . .	10.6	4.8	8.2	8.0	8.2	11.2	5.2	10.0	9.4	4.6	8.0
9. . . .	9.8	7.2	12.0	6.4	8.2	11.2	6.8	6.8	8.0	4.8	8.1
10. . . .	8.6	9.8	8.8	8.6	6.2	10.0	7.8	7.2	10.0	6.2	8.3

clearly shown. All time records in the transfer series are considerably lower than those for the first three practice trials given in column one.

However, the amount of practice effect transferred can be stated only with certain qualifications. Ewert [3] and Bray [4] have shown that the amount of transfer from one muscle group to another cannot be measured without the use of a control group. In our investigation Group A, trained with the right hand, acted as a control group on the transfer from left foot to right hand in Groups B and C. Similarly Groups

[3] P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 1926, 33, 235–249.

[4] C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 1928, 11, 443–445.

TABLE IV

INITIAL PRACTICE TRIALS AND TRANSFER TRIALS FOR GROUPS A, B, AND C. THE LEFT HAND COLUMN GIVES THE FIRST THREE OF 100 PRACTICE TRIALS, THE OTHER THREE COLUMNS THE TRANSFER TRIALS. EACH NUMBER IS THE AVERAGE OF TEN TRIALS

Group A

Trials	Practice Series	Transfer Series		
	R.H.	L.F.	R.F.	L.H.
1.....	35.9	14.7	11.7	8.2
2.....	24.7	16.9	10.3	7.4
3.....	18.8	11.3	10.0	6.5

Group B

	L.F.	R.H.	L.H.	R.F.
1.....	38.5	12.6	9.4	10.3
2.....	28.6	9.7	7.9	9.0
3.....	24.9	7.8	7.4	8.9

Group C

	L.F.	R.F.	L.H.	R.H.
1.....	37.2	9.7	9.5	11.9
2.....	32.3	9.1	7.9	9.5
3.....	26.7	8.4	7.7	8.1

B and C, practiced with the left foot, are control groups for transfer from right hand to left foot in Group A. The use of control groups of course assumes that the groups are approximately equal. However, such approximate equality seems a fair inference from the results of the trials without the mirror (Table I) and the practice series for Groups B and C shown in Fig. 4.

One measure of transfer is percentage of time saved on the first trial. To get this we find the improvement in speed of the left foot after one hundred trials direct practice, by subtracting the average time for the last ten trials from the time for the first trial. For Group B this is 32 seconds (38.5 − 6.5), and for Group C it is 31.2 seconds (37.2 − 6). The average improvement for the two groups by direct practice is thus 31.6 seconds. To find the time saved by transfer we compare Groups B and C with Group A. The

average of Groups B and C in the first trial of the practice series with the left foot is 37.85 seconds. Subtracting from this number the initial speed of the left foot in Group A's transfer series, we find the time saved in the latter due to previous practice with the right hand to be 23.15 seconds (37.85 − 14.7). The percentage of the direct gain saved by the transfer group is 23.15 ÷ 31.6, or 73 percent. A like computation shows that the transfer to the right hand from one hundred trials practice with the left foot (Groups B and C) is 76 percent of the total improvement in the speed of the right hand (Group A) from direct practice.

An important secondary problem concerns the relative amount of transfer to the different muscle groups. Referring to Table IV, we see that after training with the *right* hand (Group A) the *right* foot records are much better than those of the left foot. Similarly after practice with the *left* foot (Groups B and C) the transfer is greater to *left* hand than right hand. Also after right *hand* practice the speed of the left *hand* is about double that of the left foot (Group A). Part of the last-named difference is no doubt due to the 'absolute' superiority of hand over foot shown in Table I. However, similar results appear with Groups B and C, where the left foot has the initial one hundred trials in mirror tracing. The right foot records are better than those of the right hand except in the third trial. It would seem that when the left foot is trained, the right hand receives least benefit, and when the right hand is practiced the least transfer is to the left foot. That is, the smallest amount of transfer is to the muscle group which is both unsymmetrical and on the side opposite to the hand or foot practiced.

However, a possible alternative explanation had to be considered before the above could be asserted with any confidence. In Group B, in addition to one hundred practice trials with the left foot, the right and left hand have three trials each before the right foot is tested. Is it possible that the superiority of right foot over right hand is due to the fact that the testing of the right foot is preceded by 106 and the right hand by only 100 practice trials? What, if anything, has

the right foot gained by the additional six trials? A similar problem arose in the comparison of left hand with left foot in Group A. It was to answer these questions that Group C was tested. The procedure with Group C was in every way identical with that used for Group B, except that the transfer trials were taken in the opposite order. For Group B the order was L.F. (100 trials practice), R.H. (3 trials), L.H. (3 trials), R.F. (10 trials); with Group C the last three were reversed, L.F. (100 trials practice), R.F. (3), L.H. (3), R.H. (10). A comparison of the data from Groups B and C in Table IV shows no significant differences. The average for the right foot in Group C is 9.1 seconds as compared with 9.4 seconds for Group B, but this is balanced by the fact that the time for the right hand is also lower in Group C. The latter time is 10 seconds in Group B and 9.8 seconds in Group C.

Two questions remain to be answered. (1) Is transfer greater to the symmetrical muscle group on the opposite side, or to the muscle group on the same side of the body as the hand or foot having the initial practice? For instance, after Group A have had one hundred trials with the right hand, is the transfer greater to left hand or right foot? (2) Can we obtain a quantitative measure of the relative amount of transfer to the three muscle groups? Neither problem can be solved from the records of one group, because of the constant error introduced by the relative awkwardness of foot as compared with hand. However, the constant error can be turned into a variable error operating as often in one direction as the other, by averaging the results of Groups A and C as is done in Table V and Fig. 5. With Group A the left hand is symmetrical and on the side of the body opposite to the practiced right hand. In Group C the right foot occupies the same position relative to the initially trained left foot. Again, the right foot for Group A and the left hand in Group C are on the same side of the body as the hand or foot given the primary one hundred trials. Finally, the left foot in Group A and the right hand in Group C are controlled by muscle groups which are both opposite and unsymmetrical to the one receiving the first practice. Each of these pairs contains a

hand and a foot, a right and a left. Averaging the pairs, as is done in the right hand column of Table V, thus equalizes any influence of rightness or leftness or superiority of hand over foot, and leaves a difference which we may fairly assume to be

TABLE V

TRANSFER TRIALS FOR GROUPS A AND C

Trials	Time in Seconds			
	Group A R.H. Trained	Group C L.F. Trained	Average	Relation to Practiced Hand or Foot
	L.F.	R.H.		
1	14.7	11.9	13.3	Opposite and unsymmetrical
2	16.9	9.5	13.2	
3	11.3	8.1	9.7	
	R.F.	L.H.		
1	11.7	9.5	10.6	Same side of body
2	10.3	7.9	9.1	
3	10.0	7.7	8.85	
	L.H.	R.F.		
1	8.2	9.7	8.95	Opposite and symmetrical
2	7.4	9.1	8.25	
3	6.5	8.4	7.45	

caused by the relation of hand or foot to the hand or foot given the initial practice.

The first of the two questions raised at the beginning of the last paragraph may now be answered. The right hand column of Table V shows that the transfer is greatest to the symmetrical muscle group on the side opposite to the one practiced, next to the muscle group on the same side as the latter, and least to the non-symmetrical muscle group on the opposite side. That is, when the right hand is trained, the left hand receives most benefit, the right foot next, and the left foot least. When the left foot has the practice the order of transfer from greatest to least is right foot, left hand, right hand. The relation between the different muscle groups is most easily seen from Fig. 5. In that figure the first three trials of the practice series for Groups A, B, and C are shown

by the three upper lines. The three lower lines give the transfer series for Groups A and C, and represent the numbers from the column on the extreme right in Table V. Line D is the average for the two non-symmetrical muscle groups on the opposite side; E, the muscle groups on the same side; F, the

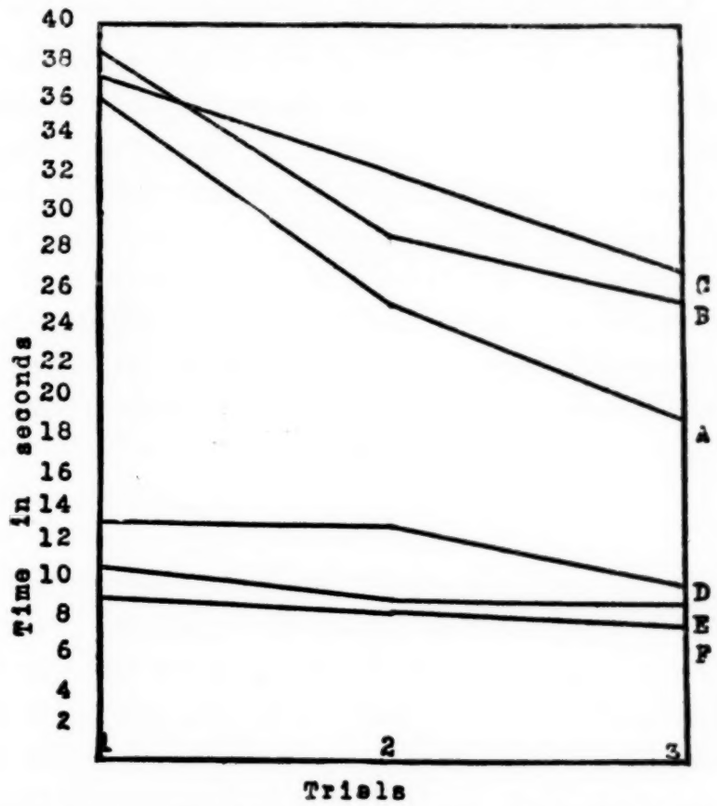

FIG. 5. Mirror tracing the star-shaped maze. *A*, *B*, *C*, first three trials of practice series with Groups A (right hand), B and C (left foot) respectively. D, E, and F, average of transfer series for Groups A and C. *D*, opposite side and non-symmetrical to trained hand or foot; *E*, same side as trained hand or foot; *F*, symmetrical and opposite to trained hand or foot.

symmetrical muscle groups on the opposite side to the trained hand or foot.

Some idea of the amount of transfer to the different muscle groups may also be obtained from inspection of Fig. 5. The amount of transfer may be expressed quantitatively by the method used previously. It will be remembered that the transfer from right hand to left foot and left foot to right hand, measured by time saved on the first transfer trial, is about 75 percent. A like computation for the first point on the two lower lines in Fig. 5 shows the time saved on the first trial to

be 84 percent and 89 percent. We may therefore say that training of one hand or foot results in a saving of initial time 89 percent with the opposite and symmetrical hand or foot, 84 percent with the hand or foot on the same side of the body, and 75 percent with the hand or foot which is both unsymmetrical and opposite to the practiced hand or foot.

Since this is the first of a projected series of studies in cross education, discussion of theory is best deferred until further data have been secured. It should be emphasized, however, that the computation of percentage of transfer in time saved on the initial trial is not intended to signify transfer in general. We cannot say that there is 75, 84, or 89 percent of transfer in any absolute sense. There are at least four ways in which amount of transfer could be estimated, and there is no reason to consider one of these more final or absolute than any of the others.

(1) Trials

(*a*) Number of trials of practice series before the practice curve reaches the point at which the transfer curve begins.

This is the only estimate of amount of transfer used by Bray. He found that after sixty trials practice with the right hand hitting at a target seen in a mirror, the number of errors made by the right foot in the first transfer trial was the same as that made by the control group in the sixth trial. The control group also used the right foot, but had no previous right hand practice. He concludes that (p. 451) "The transfer group has saved the equivalent of six trials by reason of its previous practice with the hand." Measured in this way, the gains in our experiment are much larger than those of his subjects. We can estimate the gain for both left and right hand and foot, if we assume that the practice curve for the right foot would equal the left foot curve in Fig. 4, and the left hand practice curve that of the right hand. The error involved in such an assumption is probably very slight, since the records from the preliminary practice with direct view of the maze (Table I) show only small right-left differences. Comparing the first transfer trials for either hand (Table IV)

with the practice curve for the right hand in Fig. 4, and the first transfer trials for either foot with the left foot practice curve in the same figure, we find that after one hundred trials practice with the right hand (Group A) the left foot has 'saved' 12 trials, the right foot 16 trials, and the left hand 24 trials. Similar comparisons for the other groups after one hundred trials practice with the left foot give a saving of:

	R.F.	L.H.	R.H.
Group B.....	23 trials	17 trials	13 trials
Group C.....	28 trials	16 trials	13 trials

In all three groups the relation of amount of transfer to the three muscle groups is the same as that shown in the three lower curves of Fig. 5. The transfer is greatest to the opposite and symmetrical muscle group, and the others follow in the same order. However, the percentage of transfer, in terms of trials saved at the beginning of the transfer trials after one hundred trials practice, is only 12 percent to 28 percent as compared with 75 percent to 89 percent when the measure is time saved on the first trial.

(*b*) Number of trials needed for the hand or foot in the transfer series to reach the level attained by the hand or foot by one hundred trials direct practice.

Were our results similar to Bray's on this point, such a measure could not be used. At the tenth trial (p. 450) his practice and transfer groups had reached the same level. However, if we look at Table III, we see that with Group B the right foot takes about eight seconds on and after the fourth trial. From Fig. 4 we find that the left foot required 48 trials direct practice to reach a like speed. The right foot might thus be said to have saved 44 trials on the fourth transfer trial, or that the transfer is 44 percent of the 100 practice trials. Had I been able to continue the transfer trials, I might have found that less than 56 trials would be needed to bring the right foot to the level attained by the left foot after 100 trials direct practice. If this were the case, the total number of trials saved would be more than 44. Further work on this point would be desirable.

(2) Time

(a) Time Saved on the First Trial

This is the measure used in our initial computation of amount of transfer.

(b) Total Time Saved

In computing trials saved, it should be noted that earlier trials take much longer than later ones. If therefore by one hundred trials practice we save the first forty-four trials, the saving is much more than 44 percent of the total time used in traversing the maze.

The moral of the above discussion is, of course, that measures of amount of transfer should not be stated in absolute terms. The nature of the measure must be specified.

Summary

After 100 trials practice with the right hand (10 subjects) in mirror tracing a star-shaped maze, the left foot, right foot and left hand were tested for transfer. Similarly after 100 practice trials with the left foot (20 subjects) the right hand, left hand, and right foot were tested. It was found that:

1. There was some transfer to all muscle groups.

2. The transfer was greatest to the symmetrical muscle group on the opposite side of the body, next to the muscle group on the same side, and least to the muscle group which is both unsymmetrical and opposite to the practiced hand or foot. The percentage of transfer varies with the measure used. In time saved on the first trial, it is 89 percent, 84 percent, and 75 percent respectively to each of the three muscle groups mentioned above.

3. There was no difference between the amount of transfer from hand to foot and foot to hand.

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POLYTACTIC MANUAL MOVEMENT APPARATUS

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By the term *polytactic* I wish to designate an apparatus which makes possible the learning of any one of many possible sequences of movements, these sequences all consisting of the various possible temporal combinations of a limited number of identifiable *unit* movements.

Unit manual movements may be identified in either of two ways:

1. In terms of direction and distance. A movement may be described as a movement along a straight line toward the right through a distance of 2 inches. Obviously, the muscle pattern involved in a manual movement of this sort will vary with the distance from the body of the line along which the movement is carried out. However, the visual-kinesthetic-motor training which all normal individuals bring to an experiment results in a certain equivalence of such movements carried out with different types and degrees of arm extension.

2. In terms of specific muscle groups involved in the movements. Thus a movement may be defined as a pressure on a key with a given finger. The muscle group involved will of course vary somewhat with arm position, degree of pressure, etc., but if these conditions are kept fairly constant the neuro-muscular pattern involved in successive pressures with the same finger may be treated as an identifiable unit.

In the present paper, apparatus is described which makes possible the establishment of numerous combinations of unit movements of either of the two types defined above.

The Polytactic Maze

This maze is so constructed as to have the following characteristics.[1]

[1] The history of mazes in psychology is somewhat amazing. The early maze patterns (and some of the more recent ones) were products of the landscape gardener's

1. All movement sequences consist of temporal combinations of 8 unit movements: horizontally right and left, vertically up and down, diagonally up right and left, diagonally down right and left.

2. No blindfolding or screening of the subject is necessary; the maze as a whole is visible to the subject and he can therefore use his normal eye-hand coördination habits. All parts of the maze are uniform in appearance and there are no visual cues to distinguish 'correct' from 'incorrect' paths.

3. There are no concealed stops, and an ordinary stylus with a blunt rounded end, such as is used in tapping, is employed; there is therefore no ball-and-socket joint to be steered around corners.

4. Backtracking is prevented.

5. A path of any desired length and consisting of any desired sequences of the 8 unit movements can be provided and quickly changed to an entirely different path.

6. When a subject has learned to make a certain series of movements on one part of the maze, his ability to make the same series of movements on another part can be tested without any change of the control apparatus.

7. The 'correct' path can start anywhere on the maze and end anywhere.

art rather than the results of experimental considerations. When the subject, as a penalty for one false move, is allowed to wander about for an indefinite period in a nest of blind alleys, or (what amounts to the same thing) is allowed to backtrack to an indefinite extent, and when in addition he is blindfolded, so that his normal eye-hand coordinations are excluded and so that he may have extravagant illusions as to spatial relationships (particularly when he is working his way out of a complex of blind alleys), the experimental situation is one which defies analysis. If the purpose of a maze as a psychological apparatus is to study the conditions affecting the learning of a sequence of manual movements, then wandering about in blind alleys and backtracking must be regarded as representing interruptions of the experiment; interruptions, moreover, which are variable in character. It is not astonishing that the reliability of mazes of this sort has been found to be low. If the learning of a sequence of movements is to be studied, choices of pathways must of course be provided, but once a 'wrong' move has been *initiated*, the purpose of 'blind alleys' has been fulfilled; further penetration into the 'wrong' pathway merely introduces an unanalyzable situation. Work with mazes of the traditional sort would justify Köhler's strictures on the subject of 'trial and error' learning technique, and I suspect that Kuo had something of this sort in mind when he declared that the necessity of repetitions in learning experiments was due to the experimenter's ineptness in controlling conditions.

8. When the subject initiates any movement, an automatic signal indicates whether this movement is 'right' or 'wrong.'

9. The exact path traversed is signalled to the experimenter, and errors and correct moves are permanently recorded in such a manner that a detailed time analysis is possible. In addition, total errors and total number of seconds from starting signal to last move are electrically counted.

The pattern of the maze is shown in Fig. 1. The maze

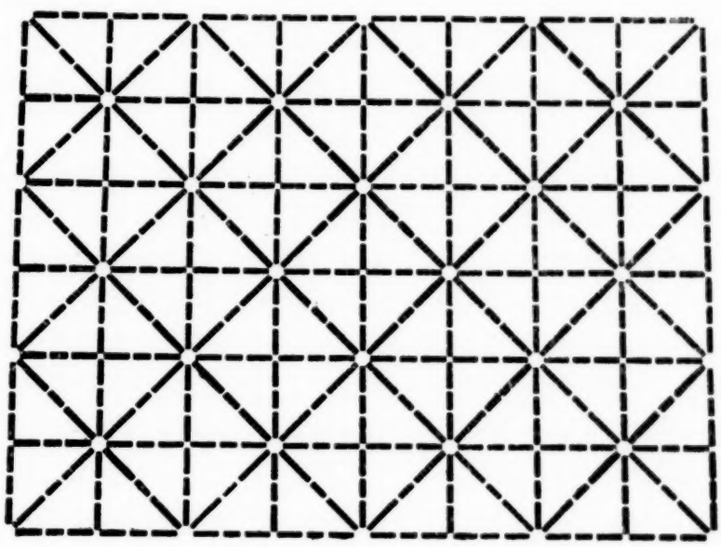

FIG. 1

foundation is a sheet of bakelite, $\frac{3}{8}'' \times 20'' \times 26''$. This sheet is screwed to a hardwood frame of which the front panel has a height of $1\frac{1}{2}''$ and the rear panel a height of $5\frac{1}{2}''$. The maze is thus given a slope which makes all parts easily accessible without undue stretching of the arm, and there is also provided in this manner an enclosed space below the maze for the electrical connections. The triangles of which the maze pattern is built up are black bakelite pieces resulting from the diagonal bisection of $2\frac{3}{8}''$ squares. The triangles are screwed to the bakelite foundation; their edges are slightly bevelled. Between these triangular pieces are the stylus grooves, of a width of $\frac{3}{16}''$. Each of these grooves contains 3 brass pieces insulated from one another by small bakelite pieces. The brass pieces are insulated from those of adjacent

grooves by square or octagonal bakelite pieces. Since the height of the brass pieces is $\frac{3}{16}''$ and that of the bakelite triangles $\frac{1}{4}''$, the depth of the grooves is $\frac{1}{16}''$. The outer borders of the maze are framed by strips of $\frac{1}{4}''$ black bakelite. All bakelite and brass pieces have threaded holes in their bottom faces and are screwed to the bakelite foundation by machine screws which enter these pieces from below; no screws are visible on the maze. The screw heads on the back of the foundation sheet serve as electric terminals for the brass pieces. The bakelite insulators are locked in with bakelite cement. All bakelite surfaces have been made dull by rubbing with valve-grinding compound; the visual effect otherwise would be somewhat hypnotic.

The maze and the control and recording apparatus are set up in separate rooms, hereafter referred to as the 'maze room' and the 'control room.' A system of mirrors, of which the primary one is located high above the subject's head and to his rear, permits general observation of the subject during an experiment. The subject wears a head-set, and verbal instructions during the course of an experiment are transmitted telephonically. To indicate to the subject the starting point on the maze for any given experiment, a double-throw switch is provided in the control room; throwing this switch in one direction dims the general illumination in the maze room and throws a spot of light from an overhead lamp-box, equipped with a lens, on the starting point; as soon as the subject has placed the stylus on this point, the switch is thrown in the opposite direction, restoring the general illumination and cutting off the lamp-box. Thus no permanent markings on the maze, which could serve as points of orientation for the subject, are necessary. Two such lamp-boxes are provided, mounted on moveable arms, so that the subject in any given experiment can be started from either of any 2 points on the maze.

Control and Recording Apparatus

As indicated above, each unit groove of the maze contains 3 insulated brass pieces, which we shall call *segments*. The 2 *end* segments of each groove on the one hand and the *center*

segment on the other are functionally different. The unit grooves are numbered, beginning at the lower left corner of the maze, from 1 to 158. In addition, the center segments are coded by letters in functional groups as follows: the *horizontal* grooves from left to right are alternately *A* and *B*; the *vertical* grooves, from above downward, are alternately *C* and *D*; the alternate *diagonal* grooves are coded similarly. Thus the grooves are coded in 8 functional groups lettered from *A* to *H*. Both the numerical and the literal coding systems are shown in Fig. 3 for the lower left square of the maze. It will be seen that from any groove only grooves of *different* code letters can be entered. The electrical connections are shown in Figs. 2 and 3.

The *end* segments of each groove are connected in common to a numbered telephone switchboard lamp (Western Electric *2G*); the bank of 158 lamps is located on the vertical switchboard in the control room.

The apparatus controlling the sequence of 'correct' moves we shall call the *stepper*. This is an electro-magnetically operated ratchet device modelled on the Ranschburg apparatus, but with all dimensions double those of the latter. The shaft carries a 10″ bakelite disc. In this disc are drilled and threaded 9 concentric rows of 60 holes each, which can receive threaded brass pegs with rounded tops. Above the disc are mounted in a radial line 9 phosphor bronze fingers. Normally these fingers press against rigid lower contacts, but as a given disc peg is rotated into position it lifts the corresponding finger, breaking the lower contact and making an upper contact. The upper and lower contacts for the outer 8 fingers are common to these electrically, and it is these 8 fingers and the sequential pattern of the corresponding disc pegs which determines the 'correct' path through the maze; the pegs can be arranged in any desired pattern. The inner or 9th finger has independent upper and lower contacts; its functions will be described presently.

The *center* segments of all grooves bearing the same code letter are connected to a pin jack on the switchboard; since there are 8 code letters, there are 8 jacks. Thence each

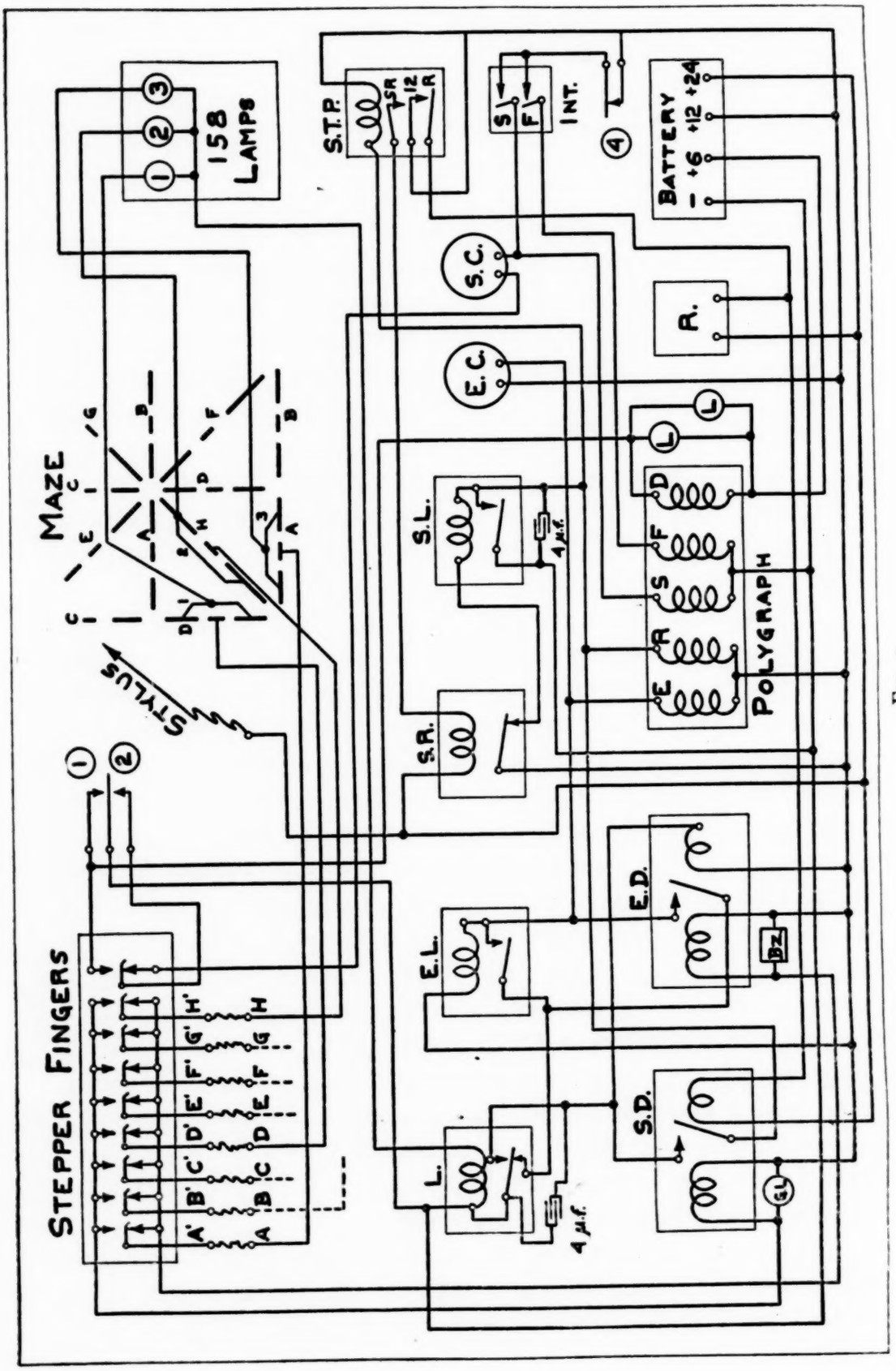
158 LAMPS
S.T.P.
INT.
BATTERY
- +6 +12 +24
S.C.
E.C.
R.
MAZE
STYLUS
S.L.
S.R.
POLYGRAPH
STEPPER FINGERS
E.L.
E.D.
L.
S.D.
BZ
GL
4 μf.

FIG. 2

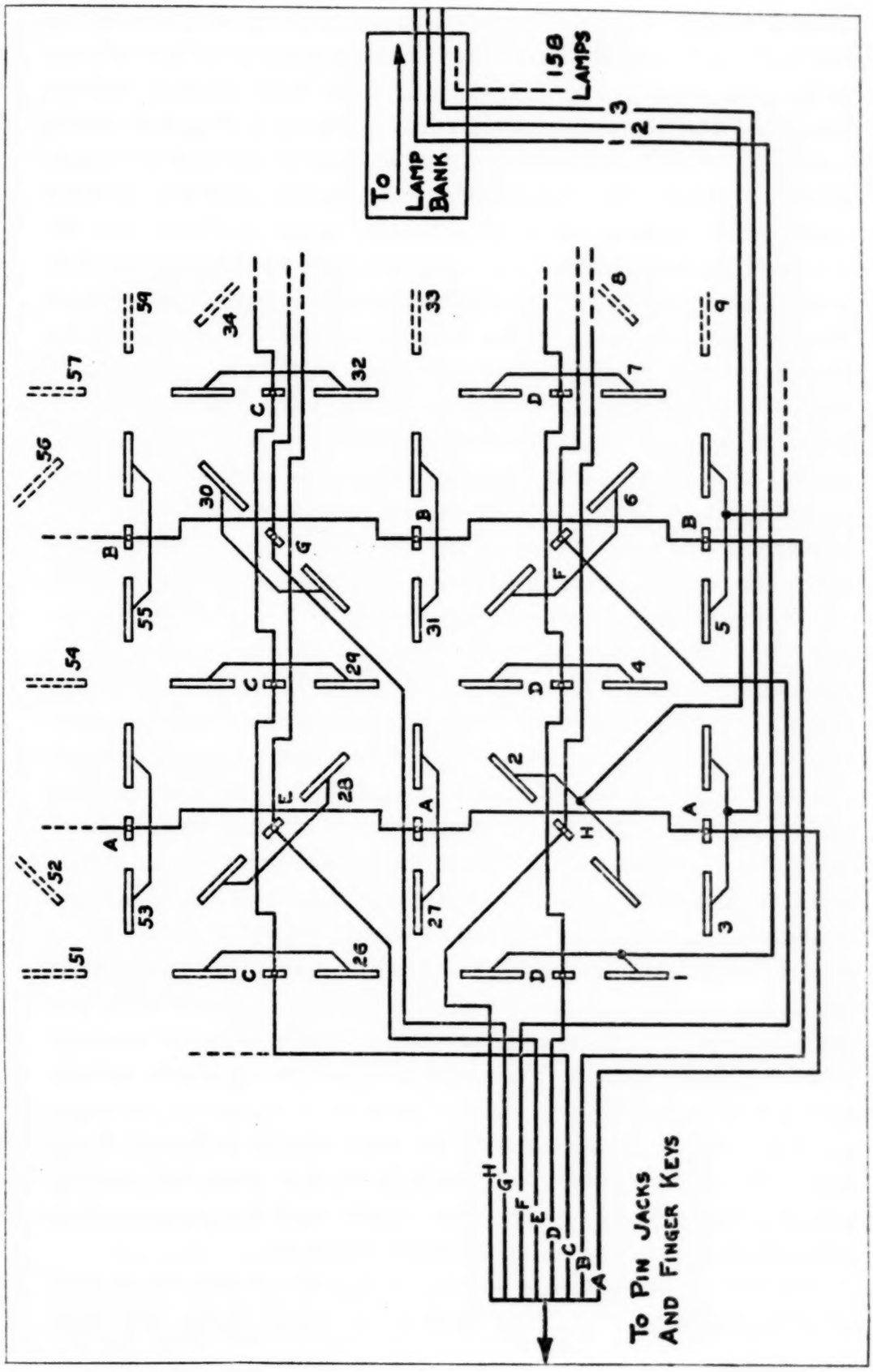
To Lamp Bank
158 Lamps
3
2
To Pin Jacks And Finger Keys
H
G
F
E
D
C
B
A

FIG. 3

circuit leads, via the corresponding pin plug, to one of the outer 8 fingers of the stepper. After a sequential pattern of pegs, and hence a sequential pattern of maze moves, is once set up on the stepper, transposition of the pin plugs results in new patterns on the maze without change of the stepper pegs. Provided that the originally set up maze pattern is kept within the outline of a square, any maze pattern can be rotated through steps of 90 degrees (or with appropriately selected original patterns, of 45 degrees) by merely transposing the 8 plugs in relation to the 8 jacks. Moreover, it is obvious that if we set up a pattern restricted, *e.g.*, to the 3 vertical columns of squares counting from the left of the maze, an identical sequence of movements can be made in a pattern shifted 1 vertical column to the right, provided we start at a symmetrically located point in each case; such a shift of a pattern requires no change of any sort in the control apparatus, since the literal coding is uniform throughout the maze.

Most of the remaining features of the electrical circuits are designed to meet 2 requirements. (1) Since the contact between stylus and groove segments is maintained entirely by the subject's pressure on the stylus, variable contacts may occur. But it is essential that when once the stylus has touched a given segment, an operation of the control and recording apparatus shall occur which cannot be repeated while the stylus is on that segment. Otherwise, if the stylus were to make and break contact several times in passing over a single segment, the stepper might make several movements, or several errors might be recorded. (2) A stepper movement must not occur while the stylus is still in contact with the segment which initiated the movement; otherwise a 'correct' groove would become 'incorrect' while the stylus is still in contact with it.

The wiring diagrams will be most easily followed if we trace the sequence of events which results from (*a*) an 'incorrect' move, and (*b*) a 'correct' move, and the events which initiate and terminate a movement sequence.

On the switchboard is located a master switch, consisting of 2 2-position lever type telephone keys; there are thus

available 4 positions, each of which gives 2 makes and 2 breaks. The 4 positions are indicated on the diagrams by encircled numbers. Only those contacts shown in Fig. 2 are at present in question. The starting point having been indicated to the subject in the manner described above, the master switch is thrown to position 1; a 6 v. current flows through the 2 lamps *L* and through the magnet of the 'duration' pen (*D*) of the polygraph. One of the lamps is located in the maze room, the other in the control room; their illumination is a 'ready' signal. The master switch is now thrown into positions 2 and 4, with the following simultaneous results: (1) the signal lamps are cut off; this is the 'go' signal; (2) the circuit through the 'duration' pen is broken, and the pen records the instant of starting; (3) an electric counter (*SC*) is connected to an interval timer (*INT*) and begins to count seconds; (4) polygraph magnets *S* and *F* are connected through the interval timer to 12 v. and their pens begin to record seconds and fifths of seconds respectively.

Suppose that the 'correct' path starts in the lower left corner of the maze, and that groove *2* represents the first 'correct' move while grooves *1* and *3* are 'incorrect.' On the stepper a peg in the 8th row is holding finger *H* in its upper position; all other fingers are in the lower position. If the stylus now enters the 'incorrect' groove *1*, its contact with the end segment establishes a 24 v. circuit through lamp *1* (of the switchboard bank in the control room) and through the *L* relay ('lamp relay'; a pony type relay with front and back contacts). The lamp lights, indicating to the experimenter the groove being entered, and the *L* relay operates; since the contact circuits of the latter are broken elsewhere, nothing further occurs. As the stylus now passes to the center segment, a circuit is established via stepper finger *D* through the line coil of the *ED* relay ('error drop'; a drop type relay with both a line and a restoring coil; Western Electric 44A), and through a buzzer (*Bz*) [2] which is coupled to the circuit of

[2] As a result of preliminary work with the apparatus, the following changes in the signal arrangements have been adopted. (a) The 'go' signal is given verbally. (b) A headphone tone of about 1000 d.v. indicates a '*correct*' move. (c) The flashing of a 40-watt frosted lamp indicates a '*wrong*' move. (d) A single stroke bell indicates

the subject's head phones. The subject hears a buzz which indicates that he has entered an 'incorrect' groove. The operation of the *ED* relay completes circuits through (*a*) the coil of the *EL* relay ('error lock'; a pony type relay with a locking circuit through its contacts); (*b*) the magnet of the 'error' pen (*E*) of the polygraph; (*c*) the magnet of an electric counter (*EC* = 'error counter'). Since *ED* is a drop relay and *EL* a locking relay, these circuits once made are maintained in spite of variable contacts of the stylus on the center segment. The subject, hearing the buzzer tone, now moves backward over the end segment by which he entered; the circuit through switchboard lamp *1* and the *L* relay is again established; the lamp signals that the stylus is leaving groove *1*, and the operation of the *L* relay breaks the circuits through the *EL* relay coil and the *ED* relay contact (and hence the circuits through polygraph magnet *E* and counter *EC*), and makes the circuit through the *ED* restoring coil (the *ED* line coil circuit having previously been broken when the stylus left the center segment). When the *ED* relay is once restored, further makes and breaks of the *L* relay contact due to variable stylus contact have no further effect.

If now the stylus enters the 'correct' groove *2*, the following events occur. Switchboard lamp No. 2 signals the contact with the end segment. Contact with the center segment completes circuits via stepper finger *H* and its *upper* contact through a green lamp (*GL*) in the maze room (indicating to the subject that this move is 'correct'), and through the line coil of the *SD* relay ('stepper drop'; a relay of the same type as the *ED* relay). A contact is made at the *SD* relay, but since the circuits through this contact are broken at the *L* relay, nothing further occurs. As the stylus now enters the farther end segment, switchboard lamp No. 2 signals this fact and the *L* relay operates. Circuits are thereby established via the upper contact of the *L* relay and the contact of the *SD* relay through (*a*) the magnet of the *R*

arrival at the goal. These arrangements, particularly the nature of the 'right' and 'wrong' signals, have been found to fit in well with the subject's set of concentration on this type of task; there are required no shifts of orientation from the maze to external sources of stimulation.

('right') pen of the polygraph; (*b*) the coil of the *SL* relay ('stepper lock'; a pony type locking relay) and the contact of the *SR* relay ('stepper release'; a slow acting relay; Western Electric 178AG); and (*c*) the magnets of the stepper (*STP*). Since *SD* is a drop relay and *SL* a locking relay, these circuits once made will be maintained in spite of variable stylus contact. The stepper armature moves, and the stepper disc rotates through 1/60 of a revolution, restoring finger *H* to the lower contact position and bringing into position a peg in another row which lifts the corresponding finger to the upper contact position, thus determining the next 'correct' move. Near extreme travel, the stepper armature makes 2 contacts (*R* and *SR*, in this order). The *R* contact establishes a circuit between the 12 and 24 v. battery terminals via contact *R* through the coil of a Ranschburg apparatus (in the control room) and also through the restoring coil of the *SD* relay. The operation of the Ranschburg apparatus indicates to the experimenter the code and serial number of the next 'correct' move, or other material required in special experiments to be described in a later publication; this material is of course printed on the cardboard disc of the Ranschburg apparatus. The operation of the *SD* restoring coil permanently breaks that branch of the stepper magnet circuit which passes through the *SD* contact. The second contact made by the stepper armature (contact *SR*) establishes a circuit through the coil of the *SR* relay. The resultant breaking of the *SR* relay contact breaks the circuit through the *SL* relay coil, and the resultant release of the *SL* relay contact breaks the circuit through the stepper magnets (the alternative circuit through the *SD* relay contact having just previously been broken, as indicated above); the stepper armature is released. Since the stepper armature is not released until the *SR* relay has operated and the *SL* relay has been released, and since the *SR* relay is slow acting, both the propelling and the stop pawls of the stepper ratchet are held in position for a sufficient fraction of a second to allow the stepper disc to come to rest; thus overtravel and backlash are prevented. All circuits are now restored to normal, and the stepper disc peg which has

moved into position under a given finger is thereby determining the next 'correct' move. If this peg has not been placed in the same row on the disc as the peg just previously in position, backtracking is prevented; if the stylus moves backward through groove *2*, the events described above for an 'incorrect' move will occur.

When the stylus enters the farther end segment of the last 'correct' groove of a given sequence, the resulting stepper movement brings into position a peg in the 9th or inner row on the stepper disc; the corresponding finger is lifted to the upper contact position, with the following results. (1) The circuit through the seconds counter *SC* is broken; the reading of this counter now indicates the total number of seconds elapsed from the starting signal to the completion of the last 'correct' move of the prescribed sequence. (2) Circuits are completed through the 2 lamps *L*, and these lamps signal, to subject and experimenter, the completion of the sequence of movements. (3) The magnet of the 'duration' pen is activated, and the moment of completion is recorded.

It will be evident that immediately upon the completion of a movement sequence the electric counters *SC* and *EC* indicate the total time and total errors respectively for the trial. The polygraph record, in conjunction with the experimenter's record of the sequence in which the various switchboard lamps flashed, makes possible a detailed time analysis of all the movements, both 'correct' and 'incorrect,' made by the subject.[3]

The Polytactic Finger Keys

For unit manual movements of type 2, telephone type non-locking pushbutton keys are provided. The contact arrangements of these keys are shown at the upper left of Fig. 4. The keys are mounted in 2 blocks, 4 keys to a block, the buttons being arranged in somewhat circular fashion to conform to the fingertip pattern of the 4 fingers of each hand. The blocks are mounted flush in a horizontal board with a space of about 18″

[3] It has been found advantageous to add another polygraph pen whose magnet is in parallel with the *L* relay; thus the instant of *entering* any groove is recorded, and there are recorded also those cases where the S enters a groove but retreats before he reaches the center segment.

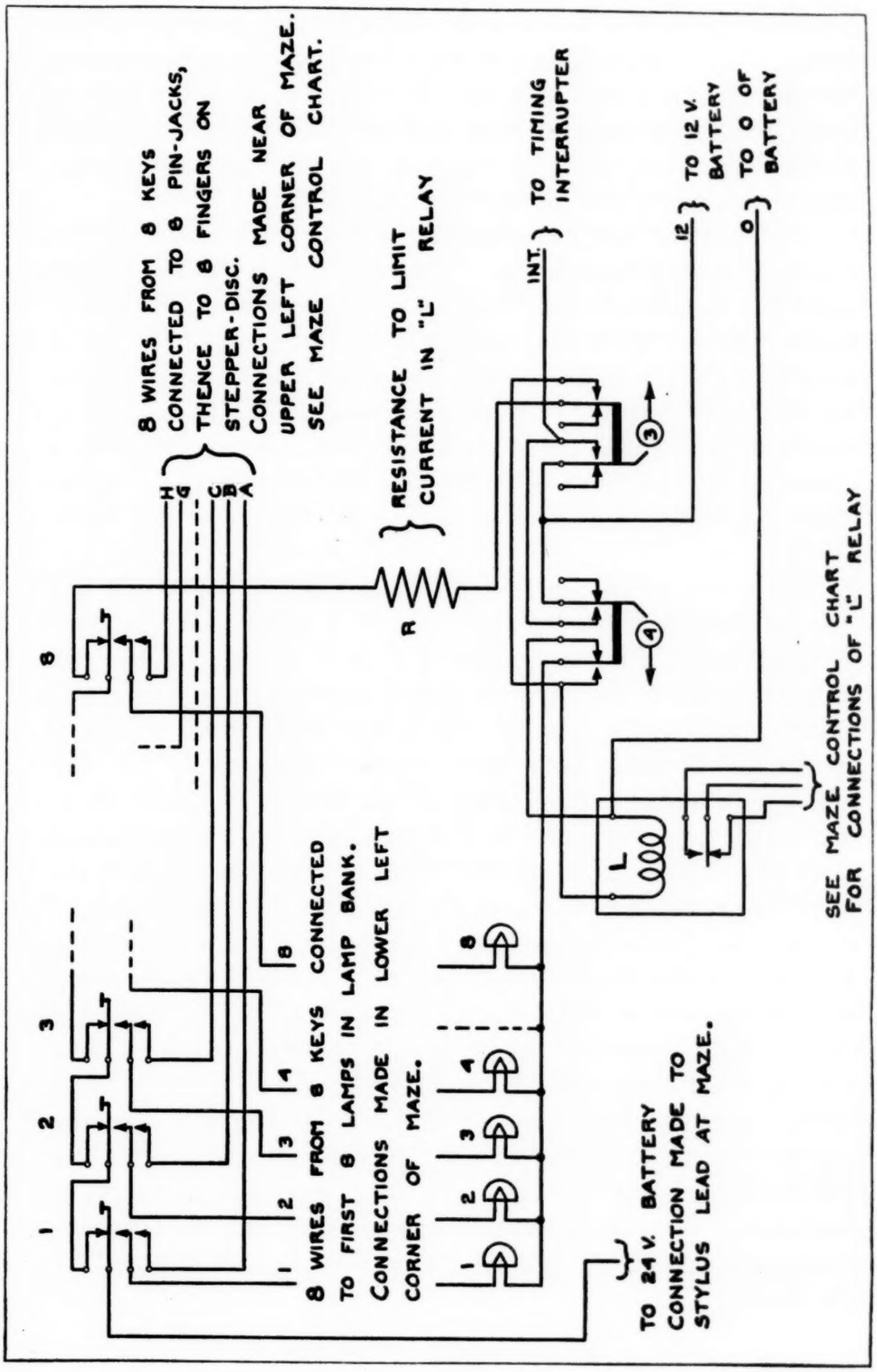
8 WIRES FROM 8 KEYS
CONNECTED TO 8 PIN-JACKS,
THENCE TO 8 FINGERS ON
STEPPER-DISC.
CONNECTIONS MADE NEAR
UPPER LEFT CORNER OF MAZE.
SEE MAZE CONTROL CHART.
RESISTANCE TO LIMIT
CURRENT IN "L" RELAY
INT. TO TIMING
INTERRUPTER
12 TO 12 V.
BATTERY
0 TO 0 OF
BATTERY
8 WIRES FROM 8 KEYS CONNECTED
TO FIRST 8 LAMPS IN LAMP BANK.
CONNECTIONS MADE IN LOWER LEFT
CORNER OF MAZE.
TO 24 V. BATTERY
CONNECTION MADE TO
STYLUS LEAD AT MAZE.
SEE MAZE CONTROL CHART
FOR CONNECTIONS OF "L" RELAY
R
L

FIG. 4

between them; the board is of sufficient size to support the arms. The button spring of each key is fairly stiff, whereas the spring just below it is very flexible; hence as the button is pushed down no additional stiffness is discriminated until the rigid bottom contact is reached; this arrangement insures that the pressure will be 'carried through.'

The control and recording apparatus is common to the maze and to the finger keys. The chief difference in arrangement is a functional separation of the *L* relay and the switchboard lamps. With the master switch in position *4*, the *L* relay and the lamps are in series, and the control apparatus is set for maze operation. With the master switch in position *3*, the lamps and the *L* relay are in independent circuits, with a resistance of 240 ohms in series with the relay in place of the lamps; the control apparatus is then set for finger key operation.

With the master key in position *3*, each key when depressed breaks 1 contact and makes 2 others successively. With all keys in normal, there is a 24 v. circuit via the top contacts of all of the keys (in series) through the resistance *R* and the coil of the *L* relay; the latter is therefore normally operated. Pressing any key results successively in the following events: (1) breaking of the circuit through the *L* relay coil; (2) completion of a circuit through a switchboard lamp of corresponding number; (3) completion of a circuit via the bottom key contact and the corresponding pin-jack and stepper disc finger through the buzzer and the line coil of the *ED* relay, if this be a 'wrong' key, or through the green lamp and the line coil of the *SD* relay if it be the 'correct' key. If the key was a 'wrong' one, its release disconnects the *ED* line coil and reactivates the *L* relay; the *EL* relay locking circuit is broken and the *ED* relay is restored. If the key was the 'correct' one, the release of the key disconnects the *SD* line coil and then makes the circuit of the *L* relay coil; the stepper mechanism is now activated and released in the manner previously described. All other functions are as described for the maze.

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DR. METZGER ON "*GESTALT UND KONTRAST.*"

In a recent issue of the *Psychologische Forschung*, Metzger [1] reviews two articles [2] emanating from this laboratory and dealing with the familiar cross-triangle designs of Wertheimer as treated experimentally by Benary.[3] While most of his criticisms can be answered only by reference to experimental investigations now in progress in the Cornell laboratories, certain minor points have been cleared up by direct communication between Metzger and the writer. On his own initiative the former is publishing a note in *Psychologische Forschung;* the writer wishes to acknowledge this courtesy and to call attention to Metzger's original article and to his note which lists the present points of agreement.

In the critical paper mentioned above Metzger has taken exception to three statements in the writer's summary of Benary's findings,[4] holding that "in der Uebersicht über Benarys Ergebnisse die Angaben *c*, *d* und *e* unrichtig sind." Metzger and the writer now agree that the following statements will render such criticism invalid:

1. Item *c*, to lose its ambiguity, must be interpreted to mean that "the effect continues to appear even on variation of the brightness-difference; and the direction of the phenomenon is unchanged."

2. To avoid ambiguity item *d* must be taken to mean only that no one particular distance between observer and object is an essential condition of the phenomenon, and not that the effect is uninfluenced by this distance.

3. Item *e* is correct as stated.

[1] W. Metzger, Gestalt und Kontrast, *Psychol. Forsch.*, 1931, 15, 374–386.

[2] W. H. Mikesell and M. Bentley Configuration and brightness contrast, *J. Exper. Psychol.*, 1930, 13, 1–23.

J. G. Jenkins, Perceptual determinants in plane designs, *J. Exper. Psychol.*, 1930, 13, 24–46.

[3] W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psychol. Forsch.*, 1924, 5, 131–142.

[4] Jenkins, *op. cit.*, 25.

4. Item *l* should be joined to item *m* and so corrected that the total will read "Brightness differences appear even when the configurations are presented in the form of pure outlines; when, under these conditions, the critical fields are darker than either figure or ground, the excluded patch is seen as *darker*."

It is a pleasure to present this note of agreement and to acknowledge the courtesy with which Metzger has acted in his attempt to carry the problem further 'auf dem Weg von den Psychologien zur Psychologie.'

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